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CONCEPTS OUT OF CONTEXT: THE PIED PIPERS OF SCIENCE *

1 *Introduction*

Even the most superficial study of the history of science shows that it is largely the history of illusions. We may divide illusions into those about adjacent sciences and those about our own. The former are not serious; we generally get the relevant parts of the facts and principles of adjacent sciences right, and when we err seriously there are exponents of the maligned science only too ready to correct us. But commonly accepted errors may exist about the principles of our own speciality and they may have a long life, for there is no outside corrective, and error, as Stefansson [1] has pointed out, has great vitality.

The particular type of error with which we are concerned is the transference of a concept into a field in which it is inapplicable; the more useful the concept in its own field the greater the danger. Just as a cynic can assess roughly the eminence of a scientist by the length of time for which his theories are able to hold up the development of science after his death, so the value of concepts, in their own field, is measurable by the amount of harm they do when it is assumed that they apply in others. These assumptions are all the more dangerous because the fact that they are being made is often not recognised. There are many examples but three will serve to illustrate the point: species, purity, and efficiency.

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* Received 1. iv. 51

[1] V. Stefansson, *The Standardisation of Error,* London, 1928

2 *The Concept of Species*

Animals and plants were divided into a number of species and classified before the idea of evolution had been properly formulated, let alone accepted. In retrospect we see that, although Linnaeus preceded Darwin, it is the evolutionary origin of species that gives the Linnaean system its logical justification. Where classification is satisfactory, species that are close together in the classification share a nearer common ancestor than species further apart in the classification. It is only if we assume special creation and relatively constant inheritance, as Cuvier and Linnaeus did, that we can expect always to be able to fit an organism into an unequivocally labelled pigeon hole. Nowadays most thoughtful biologists realise that the evolutionary origin of species is bound to give intermediate types and there may be a transient continuity between adjacent species. But anyone combatively aware of this in the eighteenth century was a nuisance. Then the most useful approach was a certainty that this is this and that that, and that the trouble with the small proportion of doubtful specimens was simply due to lack of knowledge.

Now, just as the limitations of the species concept are being recognised by students of the metabionta, and taxonomical squabbles are declining, a tendency is developing to adopt Linnaean type classification in fields where evolutionary origin of species is uncertain, as with bacteria, or improbable, as with viruses. In spite of the value of the species concept it is not necessarily applicable here. Taxonomy defines morphological categories and not identities nor biochemical similarities. The qualities most readily observed in complex organisms, and so used in their classification, depend on a pattern of units making up a peculiarity of shape or construction. It is the number of components in this peculiarity that makes its accidental appearance in an unrelated species, or its reappearance if once lost (Dollo's Law), improbable. There is no similar improbability in a unit process ; rather the converse. The way in which a thing has happened before is the most probable way for it to happen again. In single celled organisms the qualities used in classification may depend on a single enzyme process, but enzymes appear and disappear as a result of mutation or environmental changes with a readiness that makes a Linnaean type classification based on them something of a farce. The same restriction applies in the metabionta. A simple enzymic peculiarity is seldom given taxonomic significance. Thus the class mammalia is united, not by the capacity of the female to give milk—this is a capacity that it shares with pigeons, which apparently use a similar biochemical mechanism—but by the use of a more or less uniform structural mechanism for coping with milk secretion. Dollo's law is a statement about the distribution and coordination of many enzymes and hormones, and not about the presence or absence of one.

Bacteria clearly develop from other bacteria so that a type of evolution is possible. Nevertheless, the qualities we see depend so much on single enzymes that the concept of a species has doubtful applicability. With viruses the position is even worse. There is no reason to assume that the smaller viruses are descended from parent viruses. What we know about them is compatible with the idea that they are the result of a derangement of the nucleoprotein metabolism of the host cell that is brought about by a nucleoprotein. They are dependent on the pre-existing protein synthesising machinery of the host. There is already evidence[1] that several different nucleoproteins may result from this upset and the number initially made may be very large, for there are probably scavenging mechanisms in the host cell that destroy most aberrant proteins. Among the other qualities that a substance must have before it can act as a virus, sufficient robustness to withstand the complex of enzyme actions in the cell is prominent. An aberration appearing accidentally (that is through the incursion of an at present unpredictable agent such as thermal agitation or cosmic rays) will sometimes have the qualities necessary to start a virus infection. And, just as the same mutation can occur over and over again in a species, so it is reasonable to assume that the same infective state will arise repeatedly. If viruses should have an origin of this accidental sort, a classification of them should have more in common with the classifications, if any, used for hallucinations or printers' errors than with those used for animals. But virus diseases are generally discovered and studied, in the first place, by people accustomed to handling large plants and animals. They classify their normal experimental material with success along Linnaean lines and so assume, in accordance with the habit surveyed in this paper, that viruses will be classifiable in the same way.

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[1] F. C. Bawden and N. W. Pirie, 'Some Factors affecting the Activation of Virus Preparations made from Tobacco Leaves infected with a Tobacco Necrosis Virus,' *Journal of General Microbiology*, 1950, 4, 464 ; 'The Varieties of Macromolecules in Extracts from Virus-infected Plants,' *Viruses 1950*, ed. M. Delbruck, California Institute of Technology, March 1950

It is reasonable to assume that the taxonomy of the smaller viruses will turn out to be a branch of protein chemistry ; their classification can therefore only be undertaken when we have taken the first step of producing a rational classification of the proteins and this has not yet been done. When we classify the larger viruses the type of enzyme activity that they can exert will have to be considered and enzymes are an uncertain guide. There is good reason to think[1] that organisms were able to make most of the enzymes of which we are aware before they had evolved far enough to leave any fossil record. The only evolution that we know anything about is that of morphological complexity and, as Young[2] has pointed out, of the ability to coordinate mechanisms so that life is possible in a hitherto uncolonised environment. It does not, so far as we know from the record, depend on the development of new biochemical capacities. It is of course only logical to assume that in the beginning there was a biochemical evolution and that species could then have been defined biochemically. But we know nothing of this phase of evolution and in the complete absence of any evidence about the evolution of enzyme mechanisms it seems unwise to use them in a system of taxonomy.

It is worth while pausing briefly to consider the operations used in defining a species. With the higher plants and animals most of the necessary work can be done with the naked eye and a pen knife ; with the *fungi imperfectae* a hand lens must be added and with the bacteria we have to use a good microscope and examine some fermentative reactions. At each stage the examination must be sufficiently detailed to give information about so many qualities (say 20–100) that sufficient permutations are possible to permit each species to have a different one. Some qualities are more useful than others and the art of taxonomy consists in finding those that give an adequate range of possibilities ; the main restriction is that they should not be much influenced by local environmental conditions. It is obvious that the operations used in defining a species must have a scale suitable to its order of magnitude. To define bacteria we need some chemistry and a microscope ; what about the viruses ? The sequence set out at the beginning of this paragraph suggests that we will need more chemistry and a better type of microscope. Only in this way can enough information be got for unequivocal definition, for viruses do not have properties amenable to study by other methods.

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[1] A. Lwoff, *L'Évolution Physiologique : Études des Pertes de Fonctions chez les Micro-organismes*, Paris, 1943 ; N. W. Pirie, 'The Nature and Development of Life and of our Ideas about It,' *Modern Quarterly*, 1948, 3, 82

[2] J. Z. Young, *The Life of Vertebrates*, London, 1950

The contention here is not that viruses and bacteria cannot be classified ; any definable group can be classified and the system of classification may well be binomial. But Linnaean classification is a special type of binomial classification because of the unequal importance of the two words. Thus most salts are named binomially and they can be grouped just as logically if all the sodium salts are considered together regardless of the acid ion as they are if all the chlorides are grouped together regardless of the metal ion. In such a field we classify from a point of view and with an end in view and any change of position or direction of view will of necessity alter the principles of a logical classification. There is no similar reciprocity in the classification of species ; a relationship exists between all species whose first name is *Brassica* or *Felis* which is not present between those whose last name is *versicolor* or *vulgaris*. A few last names do unite comparable species, e.g. *sativum* or *officinalis*, but what is then being classified is our use of the species and not its own intrinsic properties.

When we know enough to start classifying the viruses many properties will have to be considered. We can group them according to their sizes, shapes, nucleic acid content, presence of materials other than nucleic acid and protein, sharing of common antigens, etc. ; each expert will give pre-eminent importance to his speciality. Thus the crystallographer will group things together in a way that will surprise the serologist and vice versa. Each classification will be valid but it will not have the unique validity that a Linnaean classification of species derived by evolution would have. The peculiarity that distinguishes the metabionta from other things normally classified, is that an individual or species has arrived at its present position by following a unique route and the classification, in essence, defines that route. Without evolution we are offered a choice of properties in terms of which to classify and without evolution the concept of species is misleading.

3 *The Concept of Purity*

When chemistry was becoming systematised into a quantitative science one of its most important concepts was the ' pure substance.' Philosophically the idea probably had questionable Platonic origins but it got rid of a confusion of qualities and of much vagueness.

Berthollet realised that this concept of ideal purity, and uniformity of substances sharing a name, was not fully in accordance with experience in some fields, e.g. the oxides and sulphides of iron, but this was looked on as an exceptional state of affairs ; the great body of chemistry depended on the assumption that unless the position, orientation, and nature of every atom in two molecules were the same, the molecules would be distinguishable. This is substantially true of the small molecules with which ordinary chemistry deals and for the reason, predictable as soon as the ideas of Kekulé and Van't Hoff on structural chemistry had been accepted, that the minimum possible difference between two molecules was a significant percentage of the whole. Purity is a quantitative matter and the term must be used circumspectly outside its original field.[1]

When the molecules with which we are concerned have masses of, for example, 200 d,[2] the introduction of an atom of hydrogen makes an 0·5 per cent change in the mass. This is a substitution that would not normally be looked for by a molecular mass measurement because such a change would be more easily recognised by the profound difference it would bring about in the chemical properties of the substance. In practice the smallest change that would not alter the chemical properties radically would be the addition or removal of a —CH_2— group. This would make a 7 per cent change in the mass. Thus in this zone of chemistry the smallest mass difference between two components in a mixture of substances not readily separable because of chemical differences, is quantised. With isomers, that is molecules made up of the same atoms differently arranged, the differences in the shapes of the molecules generally lead to such differences in physical properties that the members of the group are separable, and where this is not so the difficulties involved in establishing purity are already well recognised. If we exclude isotopic and isomeric differences, there is a minimum value that the mass difference can have, and it is this fact that makes possible the sharp separations of ordinary chemistry and its rigorous standards of purity. As the masses of the molecules with which we are concerned increase this quantal difference between similar molecules becomes a smaller and smaller percentage of the whole mass. It only amounts to 0·1 per cent when the mass is 14,000 d and this is the mass of the smallest of the proteins.

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[1] N. W. Pirie, 'Criteria of Purity used in the Study of large Molecules of Biological Origin,' *Biological Reviews*, 1940, **15**, 377 ; 'The Development of our Ideas on the Nature of Viruses,' *British Medical Bulletin*, 1948, **5**, 329 ; 'A Biochemical Approach to Viruses,' *Nature*, 1950, **166**, 495

[2] d = Dalton unit, one sixteenth of the mass of an oxygen atom.

Thus far the conclusions are a necessary consequence of the atomic theory, but the large molecules that have been most thoroughly studied have all a biological origin. The conclusion would be unsound if there were reason to think that the synthetic mechanisms of the cell are so organised that they turn out invariable, identical replications of the molecule. There is no reason to assume this ; the cell probably has difficulties comparable to those of the scientist in telling one structure from another. The enthymeme, on which the application of the concept of purity to large molecules depends, is that a biological system has necessarily an interest in chemical identity. This is an unrecognised piece of idealism that few biochemists will, if taxed, admit to believing. It amounts to the belief that, corresponding to every defined biochemical entity, there is an ideal molecule. Every preparation may contain a proportion of molecules that are imperfect versions of the ideal and that are inseparable, or with difficulty separable, from it, but our job is to describe the ideal. This again is pure Platonism. In the absence of evidence it is only logical to assume that the population of molecules in a preparation is distributed about a series of modes which may be defined in terms of mass, activity, stability, etc. According to our interest we would separate a different ideal representative molecule from the group. But this is a difficulty inherent in Platonism, recognised or unrecognised, when it leaves the examination of simple concepts like circles and dodecahedra and starts to meddle with complex systems.

Thus, in the domain of biochemistry which is concerned with the properties of enzymes, antibodies, toxins, viruses and so on, the concept of purity, with the connotations it has in the chemistry of the small molecules, is misleading. There is a practical limit set to the significance of the statement that a macromolecular preparation is pure or even homogeneous. Preparations can be made which retain a definable activity substantially unaltered after repetition of some treatment that was used in their preparation ; this shows that all the components of the preparation have similar behaviour towards that treatment. The preparation may crystallise ; this shows that the components are isomorphous and do not differ in mass by more than 5-10 per cent. It may have only one component when studied ultracentrifugally or by electrophoresis ; this shows that no components making up more than 5-10 per cent of the whole have masses, if homogeneous, differing by more than 5-10 per cent from the median mass of the remainder. And so on. Each technique gives a quantitative answer and it is only when the uncertainty of a measurement is less than the minimum difference between possible particles in the preparation, as happens with small molecules, that it becomes possible to say, 'This preparation is pure.' From many points of view this does not matter. If all the particles in a preparation can fit into a crystal lattice the crystallographer is satisfied ; to him the preparation is homogeneous and he is not concerned if only some of the particles are infective or enzymically active. Similarly with the other methods of measurement. But to one who is interested in the structure essential for a biochemical activity, stricter standards of logic are necessary and of observation desirable. Purity cannot be demonstrated ; what can be demonstrated is that the particles in a preparation are all similar even if not the same.

The difficulty considered here has naturally been encountered before and some suggestions for improved nomenclature to cope with it have been made. Thus Brønsted [1], who was primarily concerned with molecules built up by the linking together into large aggregates of sub-units of only one or two types, coined the word 'isochemical.' Substances were 'isochemical' when their gross composition was the same but the average size of the aggregates differed. This is a special case of the general phenomenon. Urbain[2] considered the problem with which we are concerned here ; that there is no reason to think that all particles carrying a defined activity are strictly identical. He proposed the name 'homeomere' for substances of this type. This is reasonable nomenclature but the usage could be confused with that of Anaxagoras, in a somewhat similar sense, and it is not placed in apposition to any existing word for the state of affairs that normally exists, or is assumed to exist. The necessary distinction might perhaps be made if we borrowed an idea from the disputants at the time of the Arian heresy and introduced the adjective 'homoiogeneous' or 'homeogeneous' (the word has not apparently been used in any comparable sense before) to cover the state of affairs between ideal homogeneity and heterogeneity.

[1] J. N. Brønsted, 'Molekülgrösse und Phasenverteilung. 1.,' *Zeitschrift für physikalische Chemie,* Bodenstein Festband, 1931, p. 257

[2] M. G. Urbain, 'L'Homéomérie : Identité de Charactères physio-chimiques de Substances de Compositions differentes,' *Bulletin de la Societé chimique de France,* 1937, 5e ser., 4, 1612

As technique improves it will no doubt become possible to detect inhomogeneities of the order of 0·1 per cent instead of the 1 per cent which is the limit now, but even this will be of no great help if the particles on which the measurement is made weigh 1,000,000 to 50,000,000 d. One per cent of a small virus is a lump as big as haemoglobin. The presence of such a structure could well have a profound effect on the properties of a particle but it could be impossible to demonstrate its presence by physical measurements. The recognition of impossibility, like the recognition of necessity, can be the essential first step in the clarification and extension of a field of knowledge. Sometimes the impossibility is due to technical difficulties and may be only temporary but sometimes it is likely to be permanent; a recognition of the uncertainty of our knowledge of the large molecules might fertilise biochemistry as much as Heisenberg's principle fertilised physics.

It is important to avoid using this recognition of impossibility as an excuse for returning to that looseness and vagueness which can make chemists use the word ' biologist ' as a term of abuse. Fifty years ago the elastic concepts of ' protoplasm ' and ' giant molecules ' were substitutes for thought and experimentation; at that date they were not preludes to any relevant investigation. Hopkins saw this clearly and he denounced such concepts repeatedly. Even as late as 1930 his mind boggled at the growing evidence for proteins weighing up to 1,000,000 d because he recognised that rigid standards of identity could not be applied to them. He forced people to think of metabolism in terms of molecules small enough to have an ascertainable structure. Thus the foundations were laid of our knowledge of vitamins, coenzymes, and the other agents which mediate the action of metabolites on enzymes; agents to which he liked to apply the term ' tertium quid.' With advance in technique it is now possible to make confirmable statements about substances weighing 30,000 to 100,000,000 d. The significance of these statements is limited but they do restrict the range of properties that in Hopkins' day were sometimes attributed to ' protoplasm.' Proteins are now amenable to precise investigation in many directions; it is only contended here that they are not, and from *a priori* considerations cannot be, as rigidly definable as the smaller molecules. When we do not know that a certain activity is restricted to a unique structure and when we know that we could not distinguish neighbouring structures from one another, the unqualified application of the word ' pure ' is misleading.

4 *The Concept of Efficiency*

Biologists generally think teleologically but they then rephrase their ideas into a form that is both more acceptable to their fellow scientists and more productive of further work. As a last example of a misused concept we may consider efficiency, for this concept lies behind the impulse towards teleology. The argument, if it were ever expressed fully, would run more or less as follows : This organism contains a substance or structure with no obvious function—the production of this has diverted energy and metabolites from other roles—if a variety of the organism could survive as well, in the same environment, without this activity, or with less of it, it would live more economically—therefore the substance or structure must have a function for otherwise the economical variant would ultimately become dominant. In theory this is inescapable, but the advantages of economy are quantitative ; a theoretical advantage may well come up against the principle *de minimis non curat lex.*

We need not pause to consider cases where the advantage is negative, that is where the substance or structure has the merit of getting rid of a noxious bye-product, nor where it is linked, whether in the genetical sense or otherwise, with an advantageous system. These simply offer a real advantage at one remove. There are, however, other cases where, without invoking any hidden and connected advantages, we may still doubt that an apparent prodigality is sufficient disadvantage to be bred out. Thus many male animals produce millions of sperm at an ejaculation when only a few are in theory necessary. During the course of evolution various arrangements have developed that facilitate the contact of sperm and ovum and it appears that the large number of sperm, originally an advantage with random mating in a large volume of water, should no longer be necessary. Since the large number still persists some ingenuity has been expended on devising possible subsidiary functions for sperm such as the conveyance of nutrients and of enzymes that facilitate fertilisation by the few necessary sperm. It may well be that the apparent exuberance of sperm production carries with it some such advantage but this does not follow of necessity. We have to think of survival value quantitatively rather than metaphysically and recognise that the ability to achieve fertilisation with only 10^{-5} mg. of sperm instead of the 1 mg. generally needed will confer a somewhat trivial advantage to a variant in a species in which the adults weigh 10 or even 1,000 kg.

Evolution has been proceeding for so long that considerable efficiency is likely to have been achieved ; the idea that all the components of an organism have a function, although it originally gained acceptance when organisms were regarded as the products of design, is therefore valuable as a rule. It is especially valuable when the component makes up a considerable proportion of the organism. But it is a concept that must not be used blindly ; evolution does not operate like a park keeper going round deliberately looking for litter to clear up. It can only operate on quantitatively significant hindrances or on structures that depend on the proper co-action of a large group of factors. Thus cave fish do not lose their sight because the blind have any advantage in that environment but because vision is only maintained in a species by the constant weeding out of harmful mutants. When the mutant is no longer at a disadvantage, sight, given time, disappears because there are so many ways in which it can disappear. Simpler useless structures can survive for much longer.

Many other examples of apparent inefficiency in Nature could be given and they are of importance because our instinctive idea that an organism, like a housekeeper or government, should be economical, leads to the overlooking of some of the possibilities and so to the misdirection of research. We are so sure that a library or information service would be inefficient if it had no permanent records, but kept its information by continual recopying or by passing it round by word of mouth from A to B to C and back to A, that there is strong resistence to the evidence that the brain remembers in more or less this way. Similarly there was incredulity when the early isotope studies showed that there are few permanent structures in the body but that even the bones and fat depots are in a state of continual flux. But if any organism arose that managed to do these things in what looked like a more efficient and permanent way it is very unlikely that the economy would give the variant a significant advantage.

5 *Some Other Concepts*

In discussions of this type it used to be customary to take the concept of life as an example, but so much has been written on that subject during the last fifteen years that even the most naïve biologists now realise that there are difficulties in applying the concept outside the fields in which universally acceptable definition is possible. Admittedly the realisation does not go very deep ; in books and articles about viruses, for example, it is generally confined to a few words disclaiming any attempt to define life, followed by a farrago of nonsense that depends, if it depends on anything, on the assumption that life could be defined. But lip service to a principle is better than no service at all. This forbearance has not been emulated; philosophers, fools and physicists step in where those with some knowledge of the subject have enough sense not to tread and we are still assailed by articles purporting to define or elucidate the nature of life.

These are examples of the misuse of concepts that arise immediately from my own experience. More could be gathered from other contemporary fields and still more from the past. Even the most careful and productive workers are not safeguarded from this trouble. Thus the great successes that attended the interpretation of Nature in terms of matter and motion at the end of the seventeenth and beginning of the eighteenth centuries could lead astray so critical a physiologist as Borelli[1] and so sound a farmer as Jethro Tull.[2] The former studied digestion in animals, and the latter the absorbtion of nutrients by plants; both held that mechanical subdivision was alone sufficient to make food or soil particles absorbable. Neither had any significant evidence for his erroneous point of view; each held it because it was in accordance with concepts that were proving extremely fruitful in other fields.

6 *Conclusions*

Some people think that the philosophy a scientist accepts is not of very much importance; his job is to observe the phenomena. This is a gross oversimplification and it involves the subsidiary hypothesis that all scientists are fully equipped with serendipity. A sensible philosophy controlled by a relevant set of concepts saves so much research time that it can nearly act as a substitute for genius; it may be that this is what we mean by genius. Thus equipped we avoid the pseudo-problems or, more correctly, the problems that are real in adjacent fields but pseudo in our own. A scientist can have no more valuable skill than the ability to see whether the problem he is investigating exists and whether the concepts he is using are applicable. But the lure of a concept that clarifies another field is strong and many of us follow it, like the music of the Pied Piper, to our scientific detriment.

N. W. Pirie

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[1] G. A. Borelli, *De Motu Animalium*, Rome, 1680

[2] J. Tull, *The New Horse-houghing Husbandry: or an Essay on the Principles of Tillage and Vegetation*, London, 1731

OPERATIONAL DEFINITION AND ANALOGY IN PHYSICAL THEORIES *

1 *The Operational Theory*

IN this paper I propose to examine the meaning of operational definitions of physical concepts, and the extent to which they can be applied in understanding the logical status of theories. I shall discuss one theory of modern physics, Dirac's quantum mechanics, in some detail in order to show that it cannot fulfil the requirements of strict operational definition, and I shall suggest an alternative interpretation of the theory in terms of analogy.

It has for some time been generally accepted in physics that concepts occurring in physical theories must be operationally defined, and statements such as 'A physical quantity is defined by the series of operations and calculations of which it is the result'[1] are common in the literature of physics. The most explicit formulation of the theory was given by Professor Bridgman[2] whose definition of concepts was as follows :

> In general, we mean by any concept nothing more than a set of operations ; *the concept is synonymous with the corresponding set of operations.* If the concept is physical, as of length, the operations are actual physical operations, namely, those by which length is measured ; or if the concept is mental, as of mathematical continuity, the operations are mental operations, namely those by which we determine whether a given aggregate or magnitude is continuous.

In a recent series of lectures Bridgman clearly recognises that 'mental' and 'paper-and-pencil' operations constitute a large part of the meaning of certain concepts :

> It is often supposed that the operational criterion of meaning demands that the operations which give meaning to a physical concept *must* be instrumental operations. This is, I believe, palpably a mistaken point of view, for simple observation shows that physicists do profitably employ concepts the meaning of which is not to be found in

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* Received 5. iv. 51

[1] Sir Arthur Eddington, *The Mathematical Theory of Relativity*, Cambridge, 1923, p. 3

[2] P. W. Bridgman, *The Logic of Modern Physics*, New York, 1927, p. 5

> the instrumental operations of the laboratory, and which cannot be reduced to such operations without residue. . . . All these non-instrumental operations we may loosely lump together as ' mental ' operations.[1]

We may well ask, however, what are the exact requirements for an operational definition, be it instrumental, mental or verbal, which could make the operational theory of physics more than a mere rag-bag in which definitions having quite different logical status are ' loosely lumped ' together. For instance, in his early book[2] Bridgman speaks of the definition of an electric field as ' the limit of the ratio of the force to the charge ' as the charge becomes smaller and smaller. This example can be used to illustrate two difficulties which arise when we attempt to make the idea of operational definition precise :

(i) Are we to admit highly idealised (that is, practically impossible) physical operations into our definitions ? No one actually tries to measure an electric field by going there with a charge which progressively decreases, and measuring the limit of the force to the charge. There is no guarantee that any such limit exists, and it can never be shown physically whether it does or not. A limit is obviously a mathematical concept, which brings us to the second question.

(ii) What sorts of mathematical definitions are to be allowed ? For example, may we define velocity at a point by the derivative ds/dt ? And if this is allowed, may we define the electric potential at a point r, due to a medium of charge density ρ by the volume integral $\int_V \int^r \frac{\rho}{r^2}\, dr\, dV$? And so on, to any degree of complication.

In order to fix our ideas let us provisionally adopt the following two rules for operational definition :

(*a*) The definitions are to include only physical operations which are or could be *actually performed*. This restriction might seem unduly stringent, but unless a line is drawn here it is difficult to see how any line can be drawn short of Maxwell's demon who can see individual molecules and for whom therefore little difficulty about the nature of scientific concepts arises. Again, as L. L. Whyte points out,[3] at a time when existing fundamental theory is rapidly becoming inadequate to experimental advance, we require to pay special attention to the most direct measurements, that is, to those which are as nearly as possible independent of theoretical considerations. According to this requirement a definition of electric intensity as the limit of the ratio of the force to a diminishing charge would be excluded.

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[1] This *Journal*, 1951, 1, 258

[2] *Logic of Modern Physics*, p. 56

[3] This *Journal*, 1951, 1, 304

(*b*) The definitions may also include any mathematical expression so long as specified numerical methods for obtaining a result for the concept in question are given. Here again any other requirement would involve deciding which mathematical functions were to be admitted into definitions and which not. It means that implicit definitions may be admitted, for instance a function ψ may be defined by the equation $\nabla^2\psi = 0$ if a numerical method is given for determining its value to any desired degree of accuracy.

Let us illustrate these rules by applying them to the classical theory of mechanics. It is easy to see that it is possible to build up an entirely operational theory of mechanics by basing it on the experimental proposition, originally suggested by Mach : 'Bodies set opposite each other induce in each other, under certain circumstances to be specified by experimental physics, contrary accelerations in the direction of their line of junction.'[1] These accelerations could be calculated by means of a scale and stop-watch and a numerical method for obtaining the second derivative of the s, t curve. The mass ratio of the bodies might then be defined as the negative inverse ratio of their mutually induced accelerations, and force as the product of the mass of a body and its acceleration. In this way the whole of classical dynamics would be built up by pure mathematical definition, the measurement of acceleration being the only operational process involved. But it should be noticed that while these definitions are satisfactory for a system of ordinary sized particles within our experimental reach, they cannot be applied either to atomic systems or to astronomical systems where space and time cannot be measured with a scale and stop-watch, and where for instance in astronomical systems velocities may be measured directly by spectral shifts, and not by the derivative ds/dt. According to a strict theory of operational definition the concepts arrived at in these two cases, that is, velocities and accelerations in atomic and astronomical systems, are different concepts from those which have the same name in the mechanics of ordinary sized bodies, and are only given the same name for convenience because they happen to play analogous roles in the theories concerned.

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[1] Ernst Mach, *The Science of Mechanics,* La Salle, Ill. and London, 1942, p. 304

2 *Operational Definition in Modern Physics*

Now the question arises, is it possible to give operational definitions for all the concepts of branches of physics other than mechanics? There are two questions here which must be distinguished:

(i) Is it possible to express theories as they at present exist in terms of operational definitions and calculations alone so that all concepts are replaceable by their experimental equivalents?

(ii) If this is answered in the negative, we may still ask whether theories could be entirely reformulated, so as to do away with those concepts which cannot at present be operationally defined?

As regards the first question, we have seen that for the theory of mechanics the answer is yes, but only if we restrict ourselves to terrestrial mechanics of ordinary sized bodies. This is not surprising, because the concepts occurring in mechanics are not very far removed from our everyday experience of moving bodies. The real test cases are those theories which refer to phenomena which are extremely unfamiliar, namely the very large and the very small, and it is the question of the logical status of these concepts that gives rise to the whole enquiry. The discussions given by Bridgman of these theories indicate that any attempt to reduce all the concepts to strict operational definitions leads to logical circles. For instance, he asks what is the operational meaning of the statement that the diameter of the electron is 10^{-13} cm., and shows that this concept arises from the field equations of electromagnetism, while the meaning of these equations themselves in regions of atomic dimensions involves the meaning of the space coordinates which enter into them, and so again involves the concept of extremely small lengths. This, together with various other general discussions[1] of existing theories by Bridgman and others, makes it apparent that theories as they at present exist cannot be expressed entirely in terms of operational definitions.

Let us therefore consider the suggestion that theories should be reformulated so that they do not depend at any point on concepts for which there is no possible operational definition. To do this adequately would obviously be a major scientific undertaking, but the suggestion seems to have led to some misunderstanding about the significance of certain changes of method in modern physical theories. Einstein's special theory of relativity was perhaps the first to illustrate the advantages of adopting operational definitions of velocity and simultaneity, but even here it may be noted that the orthodox definition of simultaneity does not satisfy the criteria stated above, for the stationing of observers at every point of space armed with clocks and signalling apparatus is not a practically possible operation. Whatever operational meaning simultaneity has in the theory of relativity, this definition does not state it fully. However, the main purpose of this paper is to consider another theory of modern physics, namely, the quantum theory as formulated by Dirac. Here confusion has arisen over the use of the word ' observable,' and at first glance it might look as if Dirac has adopted a strictly operational point of view. Let us look at this theory in some detail.

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[1] See for example · Bridgman, *The Nature of Physical Theory*, Princeton, 1936, p. 65; N. R. Campbell, *Physics: the Elements*, Cambridge, 1920, Ch. VI; R. Carnap, *The Logical Syntax of Language*, London, 1937, p. 319; H. Dingle, *Through Science to Philosophy*, Oxford, 1937, p. 196 ff.; P. A. M. Dirac, *Quantum Mechanics*, 3rd ed., Oxford, 1947, p. vii; Eddington, *The Mathematical Theory of Relativity*, Cambridge, 1923, p. 6; G. J. Whitrow, 'Operational Analysis and the Nature of some Physical Concepts,' *Nature*, 1950, **166**, 91

3 *The Quantum Theory of Dirac*

Consider a simple experiment which is ' explained ' by the quantum theory, and see whether the observables which occur in Dirac's form of the theory are really definable in terms of operations alone. The simplest case that illustrates the point is that of hydrogen stimulated to emit radiation. As is well known, the radiation when analysed by a spectrometer gives a line spectrum whose wavelengths are given by the formula $\frac{1}{\lambda} = R\left(\frac{1}{s'^2} - \frac{1}{s^2}\right)$, where $s = 1, 2, \ldots$ and s' for the most prominent lines is 2. The expression $\frac{1}{\lambda} = R\left(\frac{1}{4} - \frac{1}{s^2}\right)$ is called the Balmer series. R is the Rydberg constant.

This equation embodies the strictly experimental facts to be explained by the theory, all the concepts occurring in it having operational definitions. The function of a theory is to exhibit this equation as a consequence of more general principles, which also yield other equations having similar operational meanings and verifications in other types of experiment. If the theory is to be strictly operational it will be necessary to show that all the concepts involved in it can be defined in terms of such experimental concepts as λ and R ; not necessarily these alone, but these together with those that appear in the other types of experiment.

Now let us imagine that we know nothing about the interpretation put upon the atomic theory, and express the theory which explains the hydrogen spectrum in a completely formal manner, that is in the form of a hypothesis consisting of a number of axioms and rules of inference from which laws can be deduced using only the axioms and rules, and a dictionary which translates the symbols of the hypothesis into experimental concepts having operational definitions. The following is a severely simplified outline of Dirac's theory.

Hypothesis

(i) Define quantities called *states*, denoted by ψ_1, ψ_2, etc. They may be added to each other, and multiplied by complex numbers, to give quantities which are also states.

(ii) Define quantities called *linear operators* or *dynamical variables* denoted by α etc. and defined completely by

$$\psi_2 = \alpha\psi_1 \text{ for every } \psi_1.$$

They satisfy $\alpha(\psi_1 + \psi_2) = \alpha\psi_1 + \alpha\psi_2$

$$\alpha(c\psi) = c\alpha\psi, \text{ where } c \text{ is any number,}$$

but are not commutative for multiplication, i.e. $\alpha\beta \neq \beta\alpha$.

(iii) Define *eigenstates* and *eigenvalues* as solutions of $\alpha\psi = \alpha'\psi$ where α' is a number called the eigenvalue of α, and ψ is an eigenstate of α.

If one state is an eigenstate of several dynamical variables it is called a *simultaneous eigenstate* of those variables.

(iv) An *observable* is a dynamical variable whose eigenstates form a set of states on which all other states are linearly dependent.

(v) A *complete set of commuting observables* is a set which commute with each other and for which there is only one simultaneous eigenstate belonging to any set of eigenvalues of the observables.

(vi) Define a *representation* of the states. ψ is an ordinary function of q_1, q_2, q_3, where q_1, q_2, q_3 form a complete set of commuting observables, and the representatives of ψ are written $\langle q^r/\psi\rangle$ where each q^r is a set of eigenvalues of q_1, q_2, q_3. The representatives are numbers.

(vii) Define a representation of the operators similarly. The representatives of α are written $\langle q^r/\psi/q^s\rangle$, all of them forming the elements of a matrix.

(viii) ψ may be a function of an independent variable t.

(ix) Define the operator $\frac{\partial}{\partial q_1}$ by $\frac{\partial}{\partial q_1}\psi = \frac{\partial\psi}{\partial q_1}$, etc.

(x) Define a set of dynamical variables p_1, p_2, p_3 by the commutation relations

$$p_r p_s - p_s p_r = 0 \qquad\qquad q_r p_s - p_s q_r = i\hbar\delta_{rs}$$

where $\delta_{rs} = 0$ when $r = s$

$= 1$,, $r \neq s$, and $\hbar$ is a constant.

These relations are sufficient to fix p_r except for arbitrary phase factors.

(xi) Define the operator $\frac{\partial}{\partial t}$ by $\frac{\partial}{\partial t}\psi = \frac{\partial\psi}{\partial t}$ and put $i\hbar\frac{\partial}{\partial t} = H(t)$.

(xii) $q_1 = r \sin\theta \cos\phi$, $q_2 = r \sin\theta \sin\phi$, $q_3 = r\cos\theta$.

(xiii) $H = \frac{1}{2m}(p_1{}^2 + p_2{}^2 + p_3{}^2) - \frac{e^2}{r}$ where m, e are constants.

In order to see the significance of this purely formal hypothesis we will interpret each item in terms of the *classical analogy* :

(i) The *state* of a system is a generalisation of the state of a system of particles in classical dynamics. There the state is specified by the coordinates and momenta of all the particles in the system, but in quantum mechanics this specification cannot be made complete because it is assumed that any measurement disturbs the system in an unpredictable way. A state in quantum mechanics is therefore defined as ‘ an undisturbed motion that is restricted by as many conditions or data as are theoretically possible without mutual interference or contradiction.’[1]

(ii) The *dynamical variables* are the analogues of the same variables in a classical system. They are operators, because it is the operation of measurement that is significant in quantum mechanics, not the quantity measured, which in general is not a unique number.

(iii) An *eigenstate* is a state of the system for which a ‘ measurement ’ α is certain to give the value α'.

(iv) Not every dynamical variable defined as in (ii) can even theoretically be measured ; this extra condition is therefore added for *observables.*

(vi) q_1, q_2, q_3 are the Cartesian coordinates of the system.

(viii) t is the time.

(x) p_1, p_2, p_3 are the components of momentum of the particles of the system and are defined by these equations by analogy with the Poisson bracket of classical theory :

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[1] Dirac, op. cit. p. 11

$$[q_r, q_s] = 0 \quad [p_r, p_s] = 0 \quad [q_r, p_s] = \delta_{rs}$$

(xi) H is the Hamiltonian in classical mechanics, and the elimination of $\frac{d}{dt}$ in (xi) gives Schrödinger's equation of early quantum theory, which in turn depends on the analogy of Bohr's planetary atom with discrete orbits for the electrons. Thus this postulate depends on classical analogy as modified by the early quantum theory.

(xiii) This form of H is the classical form for motion in a central field, that is, the hydrogen atom is regarded as in Bohr's picture with a positive nucleus and one planetary electron.

Discrete eigenvalues of H can now be deduced from the formal hypothesis without any further physical assumptions. It is first shown that the commutation rules for p_r and $i\hbar\frac{\partial}{\partial q_r}$ are the same, and that if suitable values are taken for the arbitrary phase factors in the q's, p_r and $i\hbar\frac{\partial}{\partial q_r}$ can be equated. Using this result and the equations in (xii) and (xiii), the equation

$$i\hbar\frac{\partial}{\partial t}\psi = \mathrm{H}(t)\psi$$

in (xi) leads to a solution of the form

$$\psi = r^{-1}\chi_0(r)e^{-i\mathrm{H}'t/\hbar}S_n(\theta\phi)$$

where H' is an eigenvalue of H, $S_n(\theta\phi)$ is a spherical harmonic, and $\chi_0(r)$ is given by

$$\left\{\frac{d^2}{dr^2} - \frac{n(n+1)}{r^2} + \frac{2me^2}{\hbar^2}\cdot\frac{1}{r}\right\}\chi_0 = -\frac{2m\mathrm{H}'}{\hbar^2}\chi_0.$$

Solutions of this only exist for discrete eigenvalues of H′ when H′ is negative, these values being given by

$$\mathrm{H}' = -\frac{me^4}{2s'^2\hbar^2} \qquad s' = n+1, n+2, \ldots$$

The complete derivation of the frequency condition given by Balmer's series involves the advanced quantum theory of radiation which is too complex to outline here, but by means of it we can show that when excited in some way (for instance by incident radiation) hydrogen emits radiation whose frequency is given by

$$\nu = (\mathrm{H}'' - \mathrm{H}')/h \quad \text{where } h = 2\pi\hbar.$$

Comparing with the frequency condition

$$\frac{1}{\lambda} = \mathrm{R}\left(\frac{1}{s'^2} - \frac{1}{s''^2}\right)$$

we obtain

$$\mathrm{R} = \frac{2\pi^2 e^4 m}{h^3 c}$$

and the experimental law becomes

$$\frac{1}{\lambda} = \frac{2\pi^2 e^4 m}{h^3 c}\left(\frac{1}{s'^2} - \frac{1}{s''^2}\right).$$

4 *Operational Definition and Analogy*

Is this an operational theory, as defined in section 1? There are two types of concepts in the theory, states and operators. The operational definition appropriate for an operator is obviously that it shall be the mathematical description of an actual, practically possible measurement, and this is how Dirac speaks of the observables.[1] Reichenbach[2] qualifies this by admitting that strictly speaking no quantum mechanical occurrence is observable, since it must be inferred from macrocosmic data which are accessible to human sense organs, but adds that occurrences such as coincidences between electrons, etc., may be regarded as observable in a wider sense if they are connected with macrocosmic events by short causal chains involving only inferences of classical physics. He shows that if this account of operations is accepted, an operational definition of states (ψ-functions) can be given.[3] But is this account of operators strictly operational? In the case of our hydrogen spectrum experiment, it would imply that there are possible physical operations for measuring the distance of an electron from the nucleus of a hydrogen atom (q), and the momentum of the electron (p). By making use of instruments such as Geiger counters, Wilson cloud chambers, etc., and a great deal of technical ingenuity, it is perhaps possible that such operations might be performed with a quantity of hydrogen which is accessible to experiment.

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[1] Dirac, op. cit. p. 37

[2] H. Reichenbach, *Philosophic Foundations of Quantum Mechanics*, Berkeley, Cal., 1944, p. 20

[3] Reichenbach, op. cit. p. 91 ff.

Recent discussions make the possibility of direct measurements on this small scale seem rather remote, but it is best not to make a fundamental argument such as this rest on considerations of what is and what is not technically possible at the present time. But not even the most hardened operationalist will suggest that any such measurements are possible on material in the stars, and yet the quantum mechanical explanation of stellar spectra is as valid a part of modern physics as the explanation of spectra produced in the laboratory. Can we attempt to save the operational theory by relaxing the condition (*a*) that operations must be such as could actually be performed, and replacing it by some definition of conditions under which experience on the earth may be extrapolated to refer also to the stars? We might say in this case, for instance, that if the operations p and q for samples of hydrogen are possible, this will be regarded as the definition of p and q wherever the existence of hydrogen is postulated. If we allow this new condition, however, we are departing from statements explicitly made by Bridgman and others, according to which concepts referring to astronomical systems are not extrapolations from experience on the earth, but are essentially different concepts, to be defined only in terms of the observable phenomena connected with them. In the case of stellar radiation the only observable evidence we have is contained in the spectra, and it is clear that there is no way of measuring p and q directly, or even by means of classical inferences as suggested by Reichenbach.[1]

The second, (*b*), of our criteria for operational definition, namely the possibility of calculating numerical values for the concept from actually measurable experimental results, suggests that we may be able to calculate the representatives of an operator which, as we have seen, are elements of a matrix. Similarly, if the representatives of the states could be calculated they could be given operational definitions. To see whether this is possible it is necessary to look at the types of representation used. Schrödinger's type represents states by quantities $\langle q^r/\psi\rangle$ and operators by the matrix elements $\langle q^r/\alpha/q^s\rangle$ where q^r, q^s are eigenvalues of q which form a continuous range from $-\infty$ to $+\infty$ and are therefore infinite in number. In the momentum type of representation the elements are constructed similarly from eigenvalues of p which also form an infinite continuous range. Thus no calculation of numerical representatives is possible.

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[1] Reichenbach himself does not put forward an operational theory such as those we are examining here, so that this criticism of his definition of observability in no way affects his subsequent arguments. He would presumably accept the modification of condition (*a*) suggested above.

What then is the significance of the concepts in Dirac's theory? The answer is clearly to be found in terms of the classical analogy. It is this that gives meaning to the purely formal statements of the hypothesis and hence gives rules for manipulation of the concepts. Dirac's discussions about measurement and observability become meaningful if we realise that he has in his mind, not practically possible experimental measurements, but a highly idealised system of particles like those considered in classical dynamics, only with the difference that complete information about the positions and momenta of the particles cannot be obtained because any measurement changes the system in an unpredictable way so that no two measurements are ever made with the system in the same state, and that the particles must not be assumed to *have* definite positions and momenta when not observed, as classical particles are assumed to have. Although the theory of observables appears to be a theory about measurement, it is still far removed from measurements as they are actually carried out, and presupposes a particle analogy which is not directly given in experimental data.

The question now arises: is it possible to carry the process further and do away with all analogy in the formulation of the hypothesis? It is not possible to answer this question by any *a priori* principles; it can only be said that any such theory would be extremely unwieldy and for that reason will probably never be successful. It is difficult to see how it could be flexible enough to develop and include new observations, because one of the main functions of an analogy or model is to suggest extensions of the theory by considering extensions of the analogy, since more is known about the analogy than is known about the subject matter of the theory itself. Here the use of the word 'analogy' need not be restricted to actual physical models, but may be taken to include branches of pure mathematics with formal axioms and rules of inference whose consequences are already well worked out. A collection of observable concepts in a purely formal hypothesis suggesting no analogy with anything would consequently not suggest either any directions for its own development. It does, however, seem clear that as far as actual operations are concerned we are not justified in regarding the particle analogy as the only possible one for correlation of the experiments of atomic physics. There are indications that difficulties in the theory of quantum electro-dynamics will lead to radical changes in the present theory. In recent work Heisenberg[1] and others have attempted to extract from the current theory those aspects which are strictly observable, i.e. the discrete energy levels of stationary states and the asymptotic behaviour of wave functions at great distances from a scattering centre. It is evident that relations between these observables must appear in any future theory and it is suggested by Whyte[2] that they will appear as parameters of a ' primary field, where the term " field " does not imply a force-field, like those of classical theory, but is in some sense a direct representation of the total phenomenon.' But it has yet to be shown that the theory of this primary field will be a purely operational one, and while analogies from particles and waves may be progressively eliminated, it seems very likely that they will be replaced by other analogies, the exact nature of which we cannot yet foresee. The indications at present are that future analogies will be taken from the mathematical concepts of transformation group theory. This theory will have the same function as a physical analogy, but instead of rules of application (i.e. semantic rules) being given by natural extensions of a physical model, they will be given by the ordinary criteria of mathematics such as simplicity and generality. Thus the mathematical theory can still be called an ' analogy ' because it has, as it were, a life of its own, and is not exhaustively described by the data of observation. This being so, there is no guarantee that physical phenomena correspond to the natural development of any particular branch of mathematics, and a mathematical theory may be just as ' non-operational ' as a mechanical model.

5 *The Status of the Uncertainty Principle*

The possibility of replacing particle analogies by other types of analogy suggests a speculation about the status of the uncertainty principle in quantum theory. This is usually said to be, not a practical limitation to the accuracy of our measurements, but an inherent limitation upon our knowledge of small-scale phenomena. But if the arguments in favour of this are examined[3] they will all be found to be based on the particle analogy, and we have seen that discussions about measurements on the idealised systems of particles such as those postulated by Dirac have no direct operational significance ; in other words, it is worth examining the question whether uncertainty is an inherent limitation on our knowledge only if the particle analogy is assumed. If a theory could be formulated which did not assume the particle analogy and did not involve an inherent uncertainty, and yet did explain the same observational facts, it would have to be capable of predicting the results of actual experiments which are now unpredictable. Even the occurrence of such experiments requires a little examination, because most descriptions of them are couched in the idealised language of the analogy, and it is sometimes not clear that any actual experiment could be designed on a small enough scale to show the uncertainty at all. Usually the numbers of photons or electrons involved are so large that only the average result can be detected and this on the quantum theory is completely predictable. However, we will imagine an experiment which could actually be carried out and whose result is uncertain. Consider the diffraction of electrons at a slit, and an arrangement of Geiger counters which enables the time and place of arrival of individual electrons in a plane the other side of the slit to be recorded. All that is actually observed apart from the experimental apparatus is a series of numbers recorded by the counters which cannot be predicted by quantum mechanics, and which are interpreted in quantum mechanics as being the times of arrival of electrons at different points in a plane beyond the slit. Let us call this system A. The possibility of predicting these numbers means that it is possible to set up another apparatus B (which may be a human being with a pencil and paper) which produces the same set of numbers at a preceding time. This predictor B may be the same as A or different from A. We know that a predictor defined as A as defined above is impossible, because successive experiments with A under conditions which are the same as regards the definition of A, produce different sets of numbers. So we have to find a predictor different from A which predicts different results for the operation of A at different times. Call two experiments at different times A and A′. The difference between A and A′ is the state of the rest of the universe which is normally said to be without effect on A. Thus if a predictor B is to be possible we should have to revise our ideas about the influence of the environment on such a system as A. We cannot imagine what a new type of theory of physical phenomena not involving a particle analogy would be like, but perhaps it is not fantastic to suppose that it would involve new types of correlation and might show the isolated system assumed in experiment A to be impossible. The fact that no such inter-relations between systems like A and their environment have so far been observed might be explained by the rarity of occasions on which actual uncertainty is observed in normal physical experimentation.

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[1] W. Heisenberg, *Zeits. f. Phys.*, 1943, **120**, 513 and 673

[2] Whyte, this *Journal*, 1951, **1**, 310

[3] See for example papers by Einstein, Bohr and others in *Phys. Rev.*, 1935, **47**, 777 ; 1935, **48**, 696 ; 1936, **49**, 393

6 *Conclusion*

In conclusion, it seems that in order to understand the significance of physical theories we are led to make a distinction between two sorts of concepts—those which can be measured or calculated and therefore operationally defined, and those which enter into theories as part of the mathematical apparatus for correlating observations, and which exhibit significant analogies with more familiar processes. This distinction is implicit in several remarks made by such writers as Bridgman, Dingle, Dirac and Eddington.[1] There is no doubt that operational definitions do have an important function in modern theories, if only because they have drawn attention to these two types of concepts, and have helped to distinguish those aspects of a theory which are strictly empirical and which must therefore be explained by any satisfactory theory, and those aspects which depend upon the analogy around which the theory is constructed, and which could conceivably be replaced by another analogy. The question of the further significance of these analogies, that is, whether they are only imaginative aids in constructing formal theories of observations, or whether they can be said in any sense to describe the structure of reality, would take us into the realm of metaphysics, and that is altogether another story.

MARY HESSE

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[1] See the references cited in section 2

A NEW MODEL FOR THE MIND-BODY RELATIONSHIP*

§ 1 The Ontological Problem § 2 The Nature of Unifying Theories
§ 3 A Dualistic Model § 4 Limitations of the Model

§ 1 *The Ontological Problem*

Language analysts of the present century who have written about the mind-body relationship have confined themselves to epistemological problems, or rather to some of these. They have asked such questions as: how does one mind know about the existence and feelings of another mind from the behaviour of a body? I am not concerned with such questions here. Nor do I ask what is meant by statements about mind-body relationships. I am concerned solely with the old-fashioned ontological problem, which predominated in the philosophy of the past, particularly from Descartes onwards, but which has very rarely been discussed in recent times. My aim is to describe a model that shall express a very general relationship between minds and bodies.

Setting out from the assumption that a mind and a body are in some sense or other *two* things, the well-known theories about the possible relations between them can be divided into the following: (1) the mind acts on the body and the body acts on the mind (Interactionism or Interactionist Dualism); (2) occurrences in the mind are paralleled by occurrences in the body and *vice versa*, without Interaction (Parallelism or Parallelistic Dualism); (3) the mind is 'reducible to' the body (Epiphenomenalism or Materialistic Monism); (4) the body is 'reducible to' the mind (Idealism or Idealistic Monism); and (5) the mind and the body are 'reducible to' a third 'stuff' (Neutral Monism).

Parallelism arose because of the difficulties that beset Interactionism, and one of the main reasons for adopting monism lies in the difficulty of accepting either form of dualism. If Interactionism could be rendered more acceptable, a great part of the reason for holding any of the other theories would disappear. Probably the chief difficulty is to

* Received 8. xi. 51

understand, on the dualistic view that mind and body are 'irreducible', by what mechanism interaction between them could take place. To overcome this I wish to provide a model that shall exemplify 'irreducibility' together with interaction, and exemplify an appropriate kind of interaction.

§ 2 *The Nature of Unifying Theories*

There is a common belief that two theories can be unified in only one way, namely by 'reducing' one to the other. This can undoubtedly happen. Thus the theory of light in the hands of Maxwell was 'reduced to' his electromagnetic theory. Here 'reduced to' means the same as 'deduced from.' It is this sense that is behind abortive attempts or wishes to 'reduce' biology to physics, and so on.

But there is another and even more important sense in which two theories, say t_1 and t_2, can be unified ; and that is by means of a theory T, from which t_1 and t_2 can be separately deduced either as special cases or as approximations (these may shade into one another). There are important examples of unifying theories in this sense. Newton's theory of gravitation unified Galileo's laws of acceleration and Kepler's laws of planetary motion. Maxwell's electromagnetic theory unified the laws of Faraday and Ampère. Einstein's 1950 theory claimed to unify his gravitational theory of 1915 and electromagnetics. The important point here is that neither of the two theories, t_1 and t_2, can be deduced from the other : Kepler's laws cannot be deduced from Galileo's, nor can Galileo's from Kepler's.

The best example of unification for present purposes is afforded by electromagnetic theory. In certain conditions we may study fruitfully electrical phenomena or magnetic phenomena, without considering the other ; and we can obtain laws about either that do not refer to the other. Thus the magnetic law, that the force between two poles is proportional to their strength and decreases with the square of the distance between them, makes no reference to electrical concepts. Likewise the electrical law, that the current flowing along a conductor is proportional to the potential difference between its ends, is independent of magnetic concepts. On the other hand, there are also relations between electricity and magnetism, the discovery of which was begun by Ampère[1] and Faraday and completed by Maxwell. Ampère's law may be expressed in the form that a magnetic force induced by an electric current bears a quantitative relation to the flux of that current. Faraday's law asserts that, if a magnetic field is moved in space so that the lines of magnetic flux cut a conductor, then an electric current is set up in the conductor whose electromotive force is proportional to the rate of decrease with which the lines of flux cut the conductor. Though these laws would serve the present purpose it will be useful also to have them in general form, as given by Maxwell after he had inserted a factor unknown to Ampère or Faraday. All the relations between electricity and magnetism are contained in Maxwell's equations

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[1] We need not here consider separately the share in the discovery that is due to Biot and Savart

$$\operatorname{curl} \mathbf{H} = 4\pi \mathbf{c} + \kappa \dot{\mathcal{E}}, \quad \operatorname{curl} \mathcal{E} = - \mu \dot{\mathbf{H}}$$
$$\operatorname{div} \mathbf{H} = 0, \quad \operatorname{div} \mathcal{E} = 4\pi\rho/\kappa$$

where the vectors $\mathbf{H}$, $\mathbf{c}$, $\mathcal{E}$ are respectively the magnetic force, the current (strictly, the current density), and the electromotive force, where the curl and divergence are mathematical operators, where ρ is the volume density of electrostatic charge, and where π, κ, μ are constants. By means of these equations, if we are given the current and electromotive force we can calculate the magnetic field induced, and if we are given the magnetic field we can calculate the electromotive force and thence the current. The mutual influence is thus very marked. In general there is no current without a magnetic field, and no magnetism that does not set up a current. Whether this is absolutely general is debatable, but not important for present purposes. *Conceivably* it is quite general, and that will suffice for the construction of a model. Yet despite this mutual dependence we can often work with either electricity or magnetism separately and consider changes in one without concerning ourselves with the other, as when we are dealing with the resistance in a conductor or with the force between two magnetic poles. Sometimes, of course, it is necessary to take account of changes in, say, electricity that induce a magnetic field that in turn modifies the electric current ; but for some purposes this is not important.

We can now see that two theories, t_1 and t_2, say those of electricity and magnetism, need not lose their identity ; they are not mutually deducible ; the concepts of one are not 'reducible to' those of the other ; for we cannot work even in principle with the concepts of one alone. What this means can be seen by contrasting it with the situation where one theory t is 'reduced to' another T. Maxwell found that light was a special form of electromagnetic wave. That is to say, the theory of light is not a different kind of theory from the theory of electromagnetic phenomena. Light is simply an electromagnetic wave with a special wave-length. Naturally we retain the concept of light, because we are particularly interested in electromagnetic waves with just the wave-length that we associate with light. Nonetheless, the theory of light does lose its independent identity—it is 'reducible to' the theory of electromagnetics. (Strictly, light is associated not with one wave-length but with a small group of them.)

Let us now sum up the relations between electricity and magnetism. The phenomena of the two spheres are mutually dependent in an important sense, namely they are existentially dependent on each other. Nonetheless they are also independent in the sense that they are 'irreducible,' for their concepts are 'irreducible.' Moreover, since we can work with electricity and electrical laws without reference to magnetism, and *vice versa*, whether this is only approximately true or whether it is true only in certain conditions, we can say that each sphere possesses a certain 'autonomy.'

§ 3 *A Dualistic Model*

Whether or not electricity can exist without magnetism or magnetism without electricity is a question that does not concern us. The close linkage between the two suffices as an approximation to the model we require. Nor do we need the Maxwell unifying theory for our model. All we have to have are (i) laws connecting electrical phenomena with one another without reference to magnetism and laws connecting magnetic phenomena with one another without reference to electricity, and (ii) a way of passing from electrical phenomena to magnetic, as given by Ampère's law, and a way of passing from magnetic phenomena to electrical, as given by Faraday's law. In this way we can have two spheres of phenomena, described by two sets of concepts and ordered by two theories, together with a bridging theory. The two spheres are existentially dependent, but to a certain approximation or in certain conditions they can function independently, i.e. they are autonomous, and the two sets of concepts and the two theories are 'irreducible.' We thus have a model that may be called 'dualistic.'

I here use this term expressly for any assemblage or model that can be described only by two sets of 'irreducible' concepts, where the two associated spheres of phenomena are existentially dependent but where despite mutual influence there is a certain autonomy pertaining to each. Whatever other associations the word 'dualism' may have for the historian of philosophy are here excluded from its meaning.

The point of the model is to show that the difficulty about the concept of interaction does not appear in the electromagnetic model of interaction.

We may now turn to the mind-body problem. The generality of the dualistic model is such as to enable us to treat a body and its mind in some sense or other as *two* things. More specifically it enables us to make bridging statements between the two spheres of mind and body, some of which express universal laws. Thus we can have: 'Whenever adrenalin is injected into the blood the associated mind feels on edge' and 'Whenever a mind is made angry the level of adrenalin in its associated bloodstream rises.' Here we have laws of two-way interaction, analogous to those of Ampère and Faraday. Again, on account of the autonomy in each sphere, we can have physiological laws, such as 'Kidney disease leads to increased blood pressure,' analogous to the law of potential difference in an electrical conductor; and we can have psychological laws, such as 'Retention of learned nonsense-syllables falls off in proportion to the logarithm of the time,' analogous to the law of force between two magnetic poles. But the local autonomy in either sphere holds approximately only so long as there is (i) no considerable effect upon the other sphere and (ii) no considerable change in that sphere from any other source. We can have mutual induction phenomena. Thus 'A certain standard meal is digested in about four hours' would be true only in normal emotional conditions; if a person gets a bad fright his meal will not digest in that time or perhaps at all. Again 'Fear of failing in an examination makes a person work hard' might be subject to interference, and we might have 'His fear of failing led to duodenal ulcer, the pain of which prevented him from concentrating on his work.' Further, the model affords us the possibility of a certain kind of choice. If we wish to increase a magnetic field, we can do so either by adding more magnets or by passing an electric current, and if we wish to increase an electric current, we can do so either by introducing an additional current or by rotating a magnet. Similarly if we wish to increase the irritation a person feels at captious remarks we can do so either by making our conversation still more captious or by injecting adrenalin, and if we wish to alter the composition of a man's blood we can do so either by giving him an intravenous injection or by giving him frightening information. Put generally, we can bring about mental changes (or bodily ones) by either mental or bodily means, just as we can bring about magnetic changes (or electrical ones) by either magnetic or electrical means.

According to the model, in which magnetic phenomena cannot exist without electric phenomena and *vice versa*, mind and body have dominion status without right of secession. Thus while the model does not make a place for disembodied minds, neither does it give priority to bodies in the inventory of the fundamental furniture of the earth—though it does not provide for minds without bodies, neither does it provide for living bodies without minds.

The model does not even preclude direct action of one mind upon another mind ; for, if a mind is analogous in the model to a magnetic field, one magnetic field can exert a direct effect upon another one.

It is obvious that the model differs markedly from the classical one, put forward by Geulincx, of two synchronising though mechanically unconnected clocks.[1]

§ 4 *Limitations of the Model*

The dualistic model does not *prove* that there is interaction between mind and body. All it does is to draw attention to an unfamiliar fact, culled from electromagnetics, which displays the compatibility of 'irreducibility', existential dependence, and autonomy. This should dispel the mystery from mind-body interaction.

If anyone should object that this model dispels the mystery of mind-body interaction as little as it dispels that of electromagnetic

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[1] It might appear that quantum mechanics would provide us with an equally good model ; but this would be an error. It gives us the following important relations

$$E = h\nu, \; p = h/\lambda$$

where E, p are respectively energy and momentum, which characterise a particle, where ν, λ are respectively frequency and wave-length, which characterise a wave, and where h is a constant. These relations enable us to express quantum results wholly in terms of the concept of a particle or else wholly in terms of the concept of a wave—we can translate from one to the other at will. Now it might appear that we could express everything about a person wholly in terms of his body or else wholly in terms of his mind—that in fact all statements about a mind were translatable into statements about a body and *vice versa*. This would give us a language-analysis version of Neutral Monism. But it would not serve as a model for *interaction*. All the bridging statements and laws we wish to make connecting mind and body would have to remain unsaid.

interaction, that must be admitted ; no attempt has been made to dispel any mystery there may be about electromagnetic interaction. If this is felt to be a mystery, then all that can be claimed is that the mind-body problem has been generalised by showing that it is not *sui generis*.

The model rules out parallelism without interaction, but it does not rule out the possibility of some sort of parallelism between mental and bodily spheres. Whatever parallelism there may be, however, must be much less simple in nature than has been supposed by supporters of the traditional doctrine.

The use of the laws of Ampère, Faraday, and Maxwell may lead to disappointed hopes. It is not suggested that the bridging laws between mind and body, such as those I have used for illustration, possess anything like the great generality and definiteness of the electromagnetic laws. We may hope to attain very general and definite laws of this kind but we certainly have not yet succeeded in this. In particular, the bridging laws between mind and body are not, as Maxwell's are, statable in precise quantitative terms. It should not be overlooked, however, that many such laws are in fact quantitative, even though definite numbers cannot be given : thus courts of law commonly assume that degree of irresponsibility has a quantitative relation to the amount of alcohol consumed (though of course it is also related to other things as well, e.g. a person's individual constitution). These limitations are after all minor and do not affect the utility of the model.

It is most important to stress that, despite the model, the whole notion of mind-body interaction, consisting of ' irreducibility,' existential dependence, and local autonomy, may turn out to be untenable. All that the model is designed to do is to make it reasonable to make and take seriously certain kinds of hypotheses about minds and bodies.

J. O. Wisdom

THE CONCEPT OF ABSOLUTE EMERGENCE *

I UNDERSTAND the business of the philosophy of science to be painstakingly careful analysis of concepts, principles and methods used in science. This broad statement fails, of course, to differentiate such analysis of concepts as inevitably occurs in a developed science itself (e.g. analysis of the concepts 'simultaneity,' 'absolute motion,' 'energy,' etc. in physics) from such analysis as is more likely to be the professional concern of the philosopher of science *as philosopher*. The most natural object of distinctively philosophical analysis concerned with science would seem to be the very activity, or class of activities, defining science in general, rather than some one specific science. As it goes without saying that one such activity characteristic of science is *prediction*, the analysis of the concept of predictability is a vital task of the philosophy of science. Ability to predict is a virtue marking a good scientist ; ability to analyse clearly the concept of predictability is a virtue marking a good philosopher of science.

Now that permanent limits are set to the scientist's ability to predict by 'the very nature of things' (if I may be permitted to use loose language in paraphrasing a doctrine propounded primarily by philosophers who speak without precision) has been one of the inspiring themes of the doctrine known as the theory of 'emergent evolution.' The best known emergent evolutionists in the English tradition are probably S. Alexander, the author of *Space, Time and Deity*, and C. L. Morgan, the author of *Emergent Evolution* and, more recently, *The Emergence of Novelty*. Their central idea was that the process of evolution produces more and more complex 'levels,' like the atomic level, the level of chemical compounds, the biological level, etc. ; and that on each level new qualities emerge which are absolutely unpredictable on the basis of the laws applying to the lower levels. Perhaps the best way to impress upon the reader the urgency of analysing the meaning of the doctrine before either embracing or rejecting it, is to present a sample or two of the language through which Alexander expresses his metaphysical insight :

> The higher quality emerges from the lower level of existence and *has its roots therein*, but it emerges therefrom, and it does not belong to

* Received 30. iv. 51

> that lower level, but constitutes its possessor a new order of existent with its *special laws of behaviour*. The existence of emergent qualities thus described is something to be noted, as some would say, under the compulsion of *brute empirical fact*, or, as I should prefer to say in less harsh terms, to be accepted with the ' natural piety ' of the investigator. *It admits no explanation*. (Op. cit., vol. 2, p. 46.)
>
> A being who knew only mechanical and chemical action *could not predict* life ; he must wait till life emerged with the course of Time. A being who knew only life could not predict mind, though he might predict that combination of vital actions which has mind. . . . Now it is true, I understand, that, given the condition of the universe at a certain number of instants in terms of Space and Time, the whole future can be calculated in terms of Space and Time. But what it will be like, what qualities it shall have more than spatial and temporal ones, *he cannot know unless he knows already*, or until he lives to see. (Ibid, pp. 327-328.)

I have italicised the phrases which cry out most loudly for analysis. The following semantic discussion of the problem of emergence will not, however, make any further reference to Alexander. My point of departure will be, instead, a similar view expressed with far greater precision by a far more lucid philosopher than Alexander : C. D. Broad, in *The Mind and its Place in Nature*.

My purpose is to shed some light on the old question of emergent qualities, thrown into prominence mainly through the vitalism versus mechanism issue in the philosophy of biology, by using a semantic line of analysis which, to my knowledge, has been neglected by both parties to the dispute. It is customary to discredit the belief in absolutely unpredictable qualities on the ground that what scientific theories known today do not permit us to predict, scientific theories known tomorrow may well bring within the bounds of predictability. Thus there was a stage in chemistry when no general laws correlating molecular structure and sensible properties of compounds were known which would enable one to predict sensible properties of a hitherto unobserved compound on the basis of its molecular structure ; but such laws are now known.[1] To speak of absolute unpredictability, unpredictability once and for all, convicts one, in fact, of metaphysical obscurantism, motivated perhaps by a subconscious hostility against the faith in the omnipotence of science. Indeed, I would not wish to deny that those who, following Alexander, recommend 'natural piety' in the face of absolute novelty, most likely have no clear idea as to what they mean by such 'absolute novelty.' It seems to me, however, that this vague notion of absolute emergence can, with the help of semantic concepts, be explicated in such a way that whether a quality is emergent, is independent of the stage of scientific knowledge, but rather depends on the question whether certain predicates are *only ostensively definable.* Specifically, my purpose is to show that a law correlating a quality Q with causal conditions of its occurrence can, without obscurantism, be argued to be *a priori unpredictable* if the predicate designating Q is only ostensively definable. The concept of 'a priori predictability' here used will be defined in due time.

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[1] For a precise statement of this ' relativistic ' theory of emergence or novelty see P. Henle, ' The Status of Emergence,' *Journal of Philosophy*, 1942, and, more recently, Hempel and Oppenheim, ' Studies in the Logic of Explanation,' § 5, *Philosophy of Science*, April, 1948.

My starting-point is a distinction elaborated by Broad with great analytical effort though questionable success in the chapter 'Mechanism and its Alternatives' of *Mind and its Place in Nature*: the distinction between an emergent (or 'ultimate') law and a non-emergent (or 'reducible') law. An example by means of which Broad explains his notion of emergent law is the law connecting the properties of silver-chloride with those of silver and of chlorine and with the (presumably molecular) structure of the compound:

> . . . if we want to know the chemical (and many of the physical) properties of a chemical compound such as silver-chloride, it is absolutely necessary to study samples of *that particular compound.* It would of course (on any view) be useless merely to study silver in isolation and chlorine in isolation; for that would tell us nothing about the law of their conjoint action. . . . The essential point is that it would also be useless to study chemical compounds in general and to compare their properties with those of their elements in the hope of discovering a *general* law of composition by which the properties of *any* chemical compound could be foretold when the properties of its separate elements were known (p. 64).

Let us notice that according to the definition of an emergent law, implicit in the quoted passage, at least a necessary condition (but I suspect likewise a sufficient condition) of emergence of a law of the form 'if $C_1 \ldots Cn$, then R' (where the antecedent refers to a set of interacting components, and the consequent to a resultant of this interaction) is that instances of R must be observed before the law could be known with some probability. More exactly, if L is an emergent law in Broad's sense, then it cannot be confirmed *indirectly,* by deduction from more general laws, *before* direct confirming evidence is at hand. For short, let us say that an emergent law is *deducible only a posteriori*, or *unpredictable a priori*. The meaning of this condition will best be grasped if we consider Broad's illustration of reducible laws, i.e. laws that could be theoretically predicted with the help of a general composition law before any direct observational evidence exists. Consider the law of projectiles according to which the trajectory of a projectile is, under ideal conditions, a parabola. It is true that direct observational evidence for this law was at hand before Galileo deduced it, with the help of the parallelogram law of forces (Broad's example par excellence of a *general* composition law), from the law of freely falling bodies and the law of inertia. But it is clearly conceivable that the law should have been reached by deduction from those premises concerning the effects of isolated force components before instantial evidence was obtained (in fact, this was the case with regard to a special case of the law, namely the flight of high speed cannon balls). This, then, is what Broad would call a reducible law : it is *a priori predictable* in the sense that it is capable of prior confirmation through deduction by means of a general composition law before any confirming instances are observed. For the present purpose we may be satisfied with a denotative definition of ' general composition law ' as the kind of deductively fertile composition law illustrated by the parallelogram law. Notice that the general composition law is not claimed to be itself *a priori predictable* ; it is rather claimed to make special composition laws, like the law of projectiles, *a priori predictable*.

Now, this concept of reducible law is no sooner defined than it provokes the question : how could it ever be shown that a given law is absolutely irreducible ? In order to show this, one would have to prove that no general composition law could conceivably have been known which would have enabled a skilled scientist to predict the law *a priori*. Broad himself seems to recognise the relativity of such irreducibility to the stage of scientific knowledge at least in the case of chemistry, for the quoted passage concerning the properties of silver chloride is followed by the statement ' *so far as we know*, there is no general law of this kind.' Indeed, it is easily describable what such a general composition law of chemistry might be like : if a metal combines with an acid in solution, there results a salt and free hydrogen. This law may have been inductively derived by observing interactions of metals M, M′, M″ with acids A, A′, A″ respectively, and then be used to predict that if M‴ should react with A‴ a salt will result of which no instances have yet been observed.

Broad indeed admits that ' mechanistic ' progress in chemistry is possible to the extent that a deduction of R from $C_1 \ldots C_n$ with the help of general composition laws might be accomplished if R is a *physical* disposition of compounds, like ready solubility in water. But he holds such deduction to be in principle impossible if R is a secondary quality, i.e. a disposition to produce a sensation of a certain kind, like a pungent smell (p. 71). Does Broad have a point? I shall concede that in claiming absolute emergence for laws correlating secondary qualities with microscopic physical conditions he is inconsistent with his own definition of emergence; but I shall nevertheless argue that he came close to making a valid point overlooked by the ' relativists.' Broad claims that not even the ' mathematical archangel ' (that is, the Laplacian calculator turned to physical chemistry) could predict what NH_3 would smell like unless someone (not necessarily himself) had smelled it before. If Broad asserts the impossibility of *theoretically certain* prediction, his assertion is true but trivial: even the probability which is conferred on a special law of dynamics by deduction from the parallelogram law falls short of a maximum, since the parallelogram law itself is still capable of falsification as long as not all of its deductive consequences have been tested. But if he asserts the impossibility of ' prediction ' in the only sense in which prediction is ever possible, he is clearly wrong: just suppose that chemists had evidence suggesting the generalisation ' whenever two gases chemically combine in the volume proportion 1 : 3, the resulting compound has the smell S.' If the original evidence for this general composition law does not include observations upon the formation and properties of ammonia, the special composition law ' NH_3 has smell S ' could well have been *a priori* predicted by somebody less than a mathematical archangel before anybody had smelled that gas. It is conceivable, incidentally, that Broad confused the proposition he did assert, and which has been shown to be false, with the undeniable but irrelevant proposition that ' this gas has smell S ' cannot be logically deduced from the premise ' this gas has such a molecular structure ' alone, without the use of an additional premise asserting the correlation between structure and secondary quality.

However, there is a logical difference between the hypothetical general composition law just mentioned and the parallelogram law, which will prove to be crucial for the problem of emergence. The deduction of a special law made from the former took simply the form of deriving a substitution-instance and the predicate referring to the predicted quality was explicitly contained in the general premise. But the parallelogram law does not contain the concept of a specific type of compound motion, such as circular motion or motion along a parabola ; it only contains the concept of a specific form of functional dependence of the direction and magnitude of a compound motion upon the directions and magnitudes of the component motions. A simple way of putting the difference is this : one could understand the parallelogram law without thinking of the determinate quality of motion it may be used to predict, and therefore without ever having witnessed an instance of the predicted quality. But since the general law correlating microscopic conditions with sensations of quality Q contains the very same concept of Q as the derived substitution-instance, and Q is a simple quality of which, in Hume's language, one cannot have an 'idea' without 'antecedent impression,' the law cannot even be understood unless an instance of the predicted quality has at some time been witnessed.[1] In this sense the deductions made from such a general law do not lead to 'novelty'; when we test the deduction empirically we do not encounter a new quality the way physicists would acquaint themselves with a new quality of motion if they tested their prediction of circular motion from the parallelogram law which latter, as we might suppose, they had inductively derived from observations of rectilinear motions only. Notice that if a predicted quality Q fails to be novel in the specified sense, it does not follow that the special law 'if $C_1 \ldots C_n$, then Q' must be directly confirmed before it could be indirectly confirmed by deduction from a general composition law ; it only follows that instances of Q, which may be associated with other complexes than $C_1 \ldots C_n$ as well, must be observed before indirect confirmation is possible.

Let me clarify the point in terms of the laws correlating wave motions of the air with sound phenomena. One might be inclined to think that a general law correlating frequencies and pitches could easily be formulated which would enable *a priori* prediction of hitherto unexperienced sound phenomena in just the way in which the parallelogram law enables the prediction of so far unobserved forms of motion. Thus, let X be the highest pitch so far heard, which we shall assume to be not the highest audible pitch, and suppose that comparisons of various pitches led to the well-known law 'the higher the frequency, the higher the pitch.' With the help of this simple law we can easily predict that the pitch corresponding to a frequency higher than the frequency corresponding to X will be higher than X, before ever having heard such a pitch. If now the question should be raised whether this deduction is just like the discussed case of deduction by simple substitution, namely, a prediction of a quality which must already have been observed if the general premise is to be intelligible, the answer will have to be somewhat qualified. Strictly speaking, the quality which is being predicted is ' higher in pitch than X,' which quality is not explicitly mentioned in the general premise and therefore need not have been experienced in order for that general premise to be understood. This, then, must be admitted to be an instance of a novel (that is, so far unexperienced) quality which is not unpredictable. Yet, the reason why such *a priori* prediction is possible is that the predicate designating the quality is complex and made up of parts whose meanings are understood through ostensive definition : the meaning of the relational predicate ' higher pitch ' is understood because some, though not all, instances of this relation have been experienced, and the meaning of the proper name ' pitch X ' is understood, let us say provisionally, because pitch X has been heard. We might generalise from this example, and lay down the following principle : if a novel (that is, so far unobserved) quality Q is to admit of *a priori* prediction, then it must be complex in the sense that the expression describing it contains sub-designators (predicates and/or proper names), and these sub-designators, being understood through ostensive definition only, designate old qualities. If this principle is correct, then it follows that if there are qualities which admit of *a priori* prediction, there must also be qualities, less complex ones, which do not admit of *a priori* prediction. In terms of our illustration : one would, indeed, make a perfectly defensible claim if one said, à la Broad, that no amount of physical and physiological information could enable one to predict that a frequency increase would produce a sensation of rising pitch, if the relational predicate ' higher pitch ' admitted only of ostensive definition ; since in that case *one would not know what quality one is predicting* and the deduced statement would acquire its meaning only after verification, which is absurd.

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[1] The assumption, here involved, of only *ostensively* definable predicates is discussed in the sequel.

It is, however, of the utmost importance to realise that the concept of *a priori* unpredictability, as here analysed, is absolute only relatively to the assumption that certain descriptive predicates admit only of ostensive, not of verbal, definition. If this semantic premise fails in a given instance, the claim of *a priori* unpredictability likewise breaks down. Take, for example, the question whether it could be *a priori* predicted that a definite frequency would produce that definite pitch named ' X.' According to the present analysis, this question reduces to the question whether the meaning of the proper name ' X ' could be understood, in other words, whether the designated quality could be imagined, by someone who had never experienced the quality. Conceivably the pitch might be *described* in terms of interval-relations to already heard pitches (say, as the pitch one third higher than pitch Y, where the expression ' one third higher ' would itself be defined as meaning ' such that if Y and X occur simultaneously, the interval named " third " is heard '). If such a relational description enabled one to imagine the as yet unheard pitch, one would know what one was predicting before verification of the prediction in terms of immediate experience.

A ' relativist ' with regard to the problem of emergence might now think that his position remains unconquered after all, since it is always conceivable that a given quality-designation be understood by description rather than by ostentation. Whether a given proper name be only ostensively definable or verbally definable by means of a synonymous definite description is, indeed, not a logically decidable question but a question of psychology, specifically concerning possibilities of imagination. Just as in one logical calculus the logical constants C_1 and C_2 may be primitives and the logical constants C_3, C_4, C_5 defined, while in an alternatively constructed calculus, say, C_4 and C_1 are taken as primitives and the rest defined ; so in a descriptive language, say, phenomenological acoustics, the set of ostensively defined proper names[1] (what Russell calls ' logically proper names,' contrasted with proper names introduced as abbreviations for descriptions) is not uniquely determined. Thus, using ' C ' as ostensively defined proper name, and ' higher pitch ' (and its converse ' lower pitch ') and ' third ' as ostensively defined relational predicates, we could introduce the proper names ' E,' ' G,' ' B,' by verbal definition (for the sake of simplification, I assume the C-major scale as the field of the relation so as to be able to neglect the distinction between major and minor intervals). This is assuming the psychological possibility of imagining an as yet unsensed quality in the field of a relation R some instances of which have been sensed, on the basis of sensed qualities in the same field. If we further provide an ostensive definition for the relational predicate ' equidistant ' (pitches), we might even be able to introduce the names of all the missing pitches in the C-major scale by description. But an alternative construction of the language is clearly conceivable : we might take ' second ' and ' higher pitch ' as ostensively defined relational predicates, ' G ' as ostensively defined proper name, and then all the other proper names and interval-designations might be introduced by description without the use of the relational predicate ' equidistant.' It may perhaps be doubted whether the description ' the complex pitch resulting if a pitch a second higher than the pitch a second higher than G is sounded simultaneously with G ' would enable one to get an auditory image of a third if one had never heard a third before ; but this is a psychological question of fact.

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[1] In calling such quality-designations as ' b-flat ' proper names, I do not mean to imply, of course, that the designated pitches are particulars. I am using ' proper name ' as a term relative to a given language-level, such that the descriptive terms of lowest order in language L (terms occurring as grammatical subjects but not, in L, as grammatical predicates) are called ' proper names ' relatively to L.

If we call our first model of a language of phenomenological acoustics ' L ' and our second model ' L′ ', we can now make the following assertions : a law correlating the pitch G with a frequency is *a priori* unpredictable in L′, and so is the law correlating a definite frequency-ratio with the pitch-interval called ' second '; but those same laws are *a priori* predictable in L, if we assume that the meanings of verbally defined expressions in such languages are intelligible in the sense that the verbal definition can produce an image of the quality or relation defined, independently of any previous experience of the latter. If to say that quality Q (or relation R) is *absolutely emergent* is to say that the law correlating Q (or R) with quantitative physical conditions is *a priori* unpredictable, it follows that absolute emergence is relative to a system of semantic rules. In this respect the concept of absolute emergence turns out to be surprisingly analogous to the concepts of indefinability and indemonstrability.

Is the relativist, then, wrong in denying the existence of absolutely emergent qualities ? He is wrong if he denies the semantic truism that some descriptive terms must be given meaning by ostensive definition if it is to be possible to give meaning to any descriptive terms by verbal definition. Perhaps he is right, on the other hand, in his claim that no descriptive term is, by some obscure kind of necessity, definable by ostentation only. Even Hume, whose principle that every simple idea must be preceded by a corresponding impression is equivalent to the semantic principle that predicates designating simple qualities can become meaningful only through ostentation, allowed for the famous exception, the missing shade of blue—in fact, it could be argued that the same logic which forced him to admit this one exception should consistently have led him to allow for an infinite class of similar exceptions. However, I would like to conclude with the tentative suggestion that for every sense-field there is a general ordering relation, instances of which could not possibly be imagined antecedently to being sensed. I am referring to the relation ' higher pitch ' for the auditory sense-field, the relation ' brighter colour ' for the visual sense-field, and analogous transitive and asymmetrical ordering relations for other sense-fields or other dimensions of the same sense-fields. Whenever a verbal definition is given for a term designating an element in the field of such an ordering relation R, the relational predicate ' R ' is itself used in the definiens, together with one or more names of other elements in the field. Thus Hume's missing shade of blue, which has not been seen yet, would be verbally defined as the shade equidistant from, say, b_4 and b_6, where these are separated by a larger distance than the other consecutive elements in the series of increasingly dark shades. To say that b_5 (the missing shade) is equidistant from b_4 and b_6 evidently means that it is just as much darker than b_4 as b_6 is darker than it. But then the meaning of ' darker than ' must be understood by ostentation, and, on pain of circularity, the method of relational description by which ' b_5 ' was verbally defined is unavailable. Indeed, I do not have the faintest notion what an *analysis* of such a simple relational concept could be like. If so, then a law correlating quantitative changes in physical conditions with such changes in sensed qualities as are expressed by the terms ' darker,' ' louder,' ' higher in pitch ' etc., is absolutely emergent after all. And limits would be set to the possibility of *a priori* prediction, not by the stage of scientific progress, but by the limits of semantic analysis.

ARTHUR PAP

DISCUSSIONS

The Hypothesis of Cybernetics

> The publication of the following notes formulated some time ago on the question whether machines can be said to think may supplement the discussion of cybernetics conducted in this *Journal* by K. R. Popper (vol. 1, pp. 194–195), J. O. Wisdom (vol. 2, p. 1), D. M. MacKay (vol. 2, p. 120), F. M. R. Walshe (vol. 2, p. 161–163) and W. Mays (vol. 2, pp. 249–250.)

Since the conception of the machine involves some complications which I consider to be not strictly relevant to the argument (and I want to be as brief as possible) I shall limit myself at first to the question whether the operations of a formalised deductive system might conceivably be considered equivalent to the operations of the mind. I believe that such a suggestion involves a logical fallacy.

The declared purpose of formalisation is (1) to designate undefined terms, (2) specify unproved asserted sentences (axioms), and (3) strictly to prescribe the handling of asserted sentences, which leads to the writing down of new asserted sentences (formal proof). There prevails throughout a desire to eliminate elements that are called 'psychological.' Thus, (1) undefined terms are chosen without aiming to signify commonly understood logical relations ; (2) 'unproven asserted sentences' replace 'statements believed to be self-evident' ; and (3) the operations constituting 'formal proof' are intended to replace 'merely psychological' proofs.

I think it is logically fallacious to speak of a *complete* elimination of what have been called 'psychological' but might better be called 'unformalised' elements of deductive systems ; for :

(1) No undefined term can be introduced unless its use is first explained by ordinary speech or demonstrated by examples. An undefined term is a sign indicating the proper use which is to be made of it. The acceptance of an undefined term implies, therefore, that we believe that we know its proper use though this is not to be formally described. This proper use is a skill of which we declare ourselves to be possessed.

(2) Similarly for 'unproved asserted sentences' : the fact that they are asserted is irrelevant. Asserted by whom ? 'Asserted'

merely disguises the unavowed yet indispensable ' believed ' : namely believed by the writer and held by him to be deserving universal belief. ' Sentence ' is a disguise for statement : a statement about something. For if there is nothing that can satisfy a sentence there is no use deriving any other sentences from it; and only sentences that are statements can be satisfied or not satisfied. A ' proof ' cannot be recognised as such unless it is true that whatever satisfies the axioms from which it starts will satisfy the theorems arrived at. In accepting a statement as an axiom we express the belief that we know what does and what does not satisfy it, and that everything does. Thereby we imply the unformalised knowledge of an indefinite range of operations and of their expected result.

(3) The mere handling of symbols according to the rules of formal proof constitutes a proof only to the extent to which we accredit these operations in advance with the power of carrying conviction. But ' proof ' (as I think Ryle would say) is a success-word. The success in this case lies in the capacity of the ' proof ' to convince us (and to convince us also that others ought to share our conviction) that an implication has been demonstrated. No handling of symbols to which we refuse to award this success can be said to be a proof, no matter what pre-established rules it is said to conform to. And again the award of this success is a process which is not formalised.

Thus, I maintain that a formal system of symbols and operations functions as a deductive system only by virtue of unformalised supplements. We must know the meaning of undefined terms, understand what is stated in our axioms and believe it to be true, and acknowledge an implication in the handling of symbols by formal proof. These acts of knowing, understanding and acknowledging are not formalised : they may be jointly designated as the ' semantic operations ' of the formalised system.

Formalisation can be extended to hitherto unformalised semantic operations, but only if the resulting formal system can in its turn rely on yet unformalised semantic operations. The elimination of ' psychological elements ' by formalisation thus remains necessarily incomplete. The purpose of formalisation lies in the reduction of informal functions to what we believe to be more limited and obvious operations ; but it must not aim at their elimination.

The semantic operations attached to a formal system are functions of the mind which understands and correctly operates the system. To believe that I understand and correctly operate a formal system implies that I know how to operate its unformalised functions. Since a formal system will always require supplementation by unformalised operations, it follows that none can ever function without a person who performs these operations. A formalised deductive system is an *instrument* which requires for its logical completion a mind using the instrument in a manner not fully determined by the instrument; while the mind of the person using the instrument requires no such logical completion. A person can carry out computations with the aid of a machine (or formal system) or without it, but a computing machine cannot be said to operate except within a tripartite system:

I II III
mind→machine→things to which the machine informally refers.

Herein lies the difference between mind and machine.

If in this system we replace ' machine ' by a mind (mind (2)) we have,

I′ II′ III′
mind (1) → mind (2) → things to which mind (2) informally refers,

where the unformalised functions III′ are those of mind (1), while the experimentally observed mind (2) functions as a formalised instrument of mind (1). This is the behaviourist model of mind (2) which attributes to the experimentally observed mind an entirely different character from that required for its experimental observation by mind (1).

Such a disparity, arising merely from different temporary functions of minds, can be accepted only as representing two aspects of the mind. Both are significant and both incomplete, though in different ways. The experimentally observed aspect of the mind (mind (2)) is the brain surgeon's aspect of it. It does not include the unformalised functions of mind and lacks any element of responsible judgment, since only an observing mind (' mind (1)') can exercise such responsibility through its unformalised functions. The concept of the observed mind presupposes the observing mind but the reverse is not true. (Thus the behaviourist's intention of replacing the concept of mind by the concept of ' observed mind ' necessarily defeats itself.) On the other hand, while mind (1) can function without ever practising experimental neurology, it cannot discover within itself the facts of neurology. Mind (1) *may* be supplemented by neurology;

mind (2) *must* be supplemented by experience of consciousness and responsible judgment.

Only observing minds (minds (1)) can be supposed to communicate with each other. Inter-personal dealings like listening to or addressing a person exclude the observing of one person's mental operations by the other in the sense in which mind (1) experimentally observes mind (2).

A machine is an interpretation of an observed mind (' mind (2) ') and not of an observing mind (' mind (1) '). You can see the difference for example in the process of reaching an inductive inference. Mind (1) can reach an inductive inference and a machine can be used by it as an instrument in the process, but the inference represents its own conviction. The experimental neurologist's model, mind (2), cannot be properly said to draw an inductive inference since it lacks the elements for representing the act of reaching a conviction.

MICHAEL POLANYI

Mind-Like Behaviour in Artefacts and the Concept of Mind

IN his paper on ' Mind-Like Behaviour in Artefacts ' (this *Journal*, 1951, **2**, 105), Dr D. M. MacKay says of his mechanical analogue of the human brain that it ' shows randomness in the domain which in a human being is that of *free will ;* and behaviourally there is no reason in principle why the two should be distinguishable.' This may be quite true, but then many a reader will say : I know from my own direct experience that free will and indetermination are not the same ; accordingly, since I have free will, I conclude that I am essentially different from such a mechanical artefact, at least in some respects. Dr MacKay, however, writes that ' no reputable theologian expects to find physical laws disobeyed in the human brain.' This suggests that in Dr MacKay's opinion man is after all essentially similar to such an artefact. It is because it obeys physical laws that the artefact has no free will, but only indeterminate behaviour. If physical laws rule in the human brain in the same way as in the artefact, it is hard to see how there can be any genuine human freedom. The present writer fails to understand how Dr MacKay's conception, in spite of its complexity and ingenuity, is compatible with free will not being an illusion.

It is of interest to compare Dr MacKay's view with those Professor G. Ryle expresses in *The Concept of Mind* (London, 1949). In discussing what he calls the 'bogy of mechanism,' Professor Ryle says that physical laws 'may in one sense of the metaphorical verb, govern everything that happens, but they do not ordain everything that happens. Indeed they do not ordain anything that happens.' Professor Ryle takes the example of a game of billiards to illustrate this pronouncement. A scientific forecaster might predict from the beginning of a single stroke the movement of the balls in this particular stroke, but he will be unable to predict the whole course of the game. Thus, according to Professor Ryle, whilst the game is governed in each of its strokes by the physical laws of motion, these laws do not ordain the course of the game since the players decide what strokes they will make. We may compare this reasoning with Laplace's celebrated sentence in which he says that a sufficiently powerful intelligence knowing at a given instant the positions and velocities of all the particles constituting the physical universe would be able to predict their positions and velocities at any other instant. According to this, the bodies of the players being part of the physical universe, the whole game of billiards, including all movements of the players, would of course be completely determined by physical laws. Contrary to Professor Ryle's opinion, Laplace's laws of physics would therefore ordain everything that happens. Now it is true that the picture presented by to-day's physics is rather different from that of Laplace's, particularly with regard to the predictability of elementary events. But an examination of physics, *qua* physics, gives no indication that the new physical laws should not apply to the components of the human body, just as Laplace's laws did—Dr MacKay for instance believes that they do. Thus it would appear that Professor Ryle has simply invented a new physics, whose laws would presumably be the same as those of ordinary physics but with the important proviso that they would not apply to the bodies of men. According to Descartes, the human body was a machine, but a part of the body contained a non-mechanistic soul. According to Professor Ryle, the whole human body would be non-mechanistic. Unless the present interpretation of Professor Ryle's theory is entirely mistaken, this theory seems to be of such an *ad hoc* nature that it can hardly be called a solution of Descartes's problem.

The above remarks are not meant to detract in any way from the general value of Professor Ryle's study of the concept of mind, nor of Dr MacKay's studies in cybernetics. Their aim is merely to confront two different analyses of a problem of general interest and to show why, in the opinion of the present writer, they both leave fundamental difficulties unsolved. The first analysis accepts ordinary physical concepts, but does not appear to fit in squarely with the facts of human experience. The second starts from human experience but is led into building up an unorthodox physics.

The laws of physics are inferred from our experience, whilst our consciousness of freedom is an immediate experience. As Professor Dingle pointed out, ' it is inconceivable that experience can be refuted by deductions from experience ' (' The New Outlook in Physics,' *The Listener*, 1948, p. 761). It seems, therefore, that a close study of the scope and meaning of physical laws may help us to solve the problem of free will *versus* mechanism, but we are unlikely to succeed in doing so by merely denying their validity in certain fields when it suits our purpose.

M. H. PIRENNE

REVIEWS

THE WORLD OF FORM

The idea of form is fundamental to a very large part both of our perception and of our thinking. Indeed the more we consider it the harder it becomes to set certain limits to its place in our mental and emotional lives—artists and scientists, historians and theologians, mathematicians and philosophers are for ever seeking 'form' in one or other of its aspects. So it was a courageous undertaking on the part of Lancelot Law Whyte to sponsor a survey[1] (to which he himself contributes an introduction, a learned chronological survey and a bibliography) of the problem of form as it appears to thinkers and workers in a series of very different fields. The work originated in the idea of a guide to the 1951 Summer Exhibition of the Institute of Contemporary Arts, entitled 'Growth and Form.' But it rapidly outgrew this and assumed quite other proportions, and the volume now before us is a symposium of permanent value as a step forward in the process of finding and marking out the ground common to an enormous variety of human activity. True, it has a number of weaknesses, mostly attributable to the haste with which it had to be prepared; but to have achieved any noteworthy result at all in the short time available reflects much credit on the enthusiasm, energy and insight of the editor. Moreover, the work is above all timely, since perhaps never before have the Arts and the Sciences been so sympathetic to and understanding of one another as now.

The eleven essays can be conveniently if roughly divided into two groups, the first six dealing with the origin and significance of form in the world and the remaining five being concerned with the perception of form by animals and men. To take the first group first, we make an excellent start with Humphreys-Owen's essay on the physical principles underlying inorganic form. He points out that in the analysis of matter performed by physics we infer the existence of atoms and imagine them as occupying definite positions in space. The forces of attraction and repulsion acting between atoms and molecules may come into equilibrium in such a way that the matter contains *internal* regularity which, as in the crystalline state, confers the potentiality of producing *external* regularity such that the matter has an interesting shape. But this tendency to internal regularity is opposed by the random molecular movements known as heat motion. Thus matter possesses internal form to the extent to which this heat motion is tamed ; and so, while a gas is virtually 'formless,' a liquid surface provides us with the first manifestation of the visible form inherent in the tendency of molecules to hold together. But our 'seeing' of this or any other surface is in fact an act of averaging, for the light reflected from each atom is, of course, not individually observed. It is the merged emission from very many atoms which produces the smooth sensation of form. Although at and above the atomic level physics and sensation run roughly parallel, when we infer events within the atom we enter a world 'beyond sense,' where the word 'organisation' is more appropriate than 'form.' Thus as we proceed with the physical analysis of form, rather than arriving at smaller and smaller replicas of the form we see, form gradually vanishes and is replaced by a system of events. Moreover, because light of very small wave-lengths interacts with matter like a bullet, the disturbance which results makes it impossible to perceive or infer form or shape in the smallest physical entities. 'Only in a very limited sense can we even ascribe the property of "occupying space" to these smallest entities. To speak of electrons as outside the nucleus is just permissible, but any attempt to ascribe shape to the orbits of the electrons must fail. . . . In the deepest levels of the analysis form is the first to vanish and later even the occupation of space loses meaning.'

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[1] *Aspects of Form*, Edited by Lancelot Law Whyte, Percy Lund Humphries & Co. Ltd., London, 1951. Pp. ix + 249, 21s.

After C. C. L. Gregory's consideration of shape and distance in astronomy we come to a group of biological essays. Waddington, ranging from goose bones and wings of mutant *Drosophila* to the sculptures of Arp and Henry Moore, gives a most stimulating analysis of the wholeness of organic form as an expression of a stable internal equilibrium resulting from the interaction of many different forces. After an interlude on plant form by F. G. Gregory which, while somewhat off the main line of the argument, is an admirable, concise and up-to-date summary for the non-specialist of the mode of action of the plant hormones, we come to Needham on 'Biochemical Aspects of Form and Growth.' Here are followed up to their present position and significance some of the fundamental ideas of Koltzov, D'Arcy Thompson and Gowland Hopkins. Needham suggests that for the comprehension of the universe in terms of modern science, only two components are required ; Organisation on the one hand and Energy on the other. This is linked on to the famous aphorism of A. N. Whitehead that 'Biology is the study of the larger organisms and physics the study of the smaller ones.' The cell is a kind of chemical factory where a large number of reactions can proceed in close contiguity without becoming disorganised. The comprehension of this factory is, however, rendered vastly more difficult by the discovery (clinched by the modern technique of labelling individual atoms by the use of radio-active isotopes) that the roofs and girders of the structure, e.g. the protein and carbohydrate polymers, are not static but are in a continual flux of metabolic scrapping and replacement. Thus the very constancy of the structure is an expression of the flux of activity of the metabolic system. The foundations of morphological structure are in fact to be sought in the proteins responsible for cell structure, but these themselves are inescapably part of the metabolic process of the living cell without which they could not exist. Thus organisation seems to be the product of energy, but it in its turn appears to control and canalise the energy.

From biochemistry we come to embryology, expounded by Dalcq. Whereas Needham would seem to aver that form is scientifically inconceivable without matter, Dalcq is aware, as Needham seems not to be, of the difficulties arising from assuming that the perception of form implies that the form observed is therefore physically present in the brain, and he starts out with the statement that ' Form is both deeply material and highly spiritual.' This sets the tune of the essay and we are led by a profound and fascinating (though sometimes obscure) route to a consideration of the epistemology and indeed the whole philosophy of science. Nervous functions imply a kind of ' knowledge,' not necessarily conscious, and when we come to the highest of human intelligences with potentialities of insight into the world and capacity for inductive synthesis, we are reminded of E. Huant who in 1946 spoke of ' a property of contact, of spiritual adhesion, a kind of permeability of the universe to human thought.' Finally, after a consideration and rejection of both Mechanism and Vitalism, we are led to what the author terms ' Organicism ' (but which seems to the reviewer what might be called ' Vitalism in modern dress ') which affirms that whatever aspect of form be considered—whether physical form or morphogenesis, in evolution or in mental achievement—' the primacy of an Order, of an Idea, can always be asserted.'

Dalcq's contribution is the climax of the first part of the book, which deals with the occurrence and origin of form in nature. From the rarified philosophical heights reached by Dalcq it is a pleasant relief to descend to the warm friendly lowlands, where Cott gives us the delightful story of 'Animal form in relation to appearance,' showing what admirable insight the form and colour of animals gives us into the perceptions of animals themselves. Readers of Cott's book on *Adaptive Coloration in Animals* will know what to expect and will not be disappointed.

Konrad Lorenz on ' The Role of Gestalt Perception in Animal and Human Behaviour ' gives what will perhaps to the ' straight' biologist be the most stimulating essay of the book. No short summary can do it justice—it must be read in full. Here we can make only a few comments and criticisms. Lorenz proposes to restrict the term ' gestalt ' as Ehrenfels and Wertheimer originally did, to ' a system of universal interaction ' ; that is to say, a system in which every part influences every other part. This is in contrast to the connotation ' a mosaic of independent structures ' more recently made popular by W. Köhler. Now on a relatively superficial level in the study of animal behaviour, this distinction is undoubtedly a valuable one, as Lorenz shows convincingly. Thus there is no doubt that inborn responses of animals are usually made to relatively simple configurations, as in the female stickleback which recognises a male of the species as something with 'red below.' Moreover, these simple responses tend to be additive in their effect upon the animal which receives them—the so-called ' law of heterogeneous summation.' On the other hand, those responses which are acquired as the result of experience, as when a dog or a pet bird comes to recognise its human master or another member of its own species individually, are always based on much more subtle combinations of complex qualities which do undoubtedly partake of the nature of a true 'gestalt' and appear all-or-none in character—not additive.

But, useful as this distinction is, it is not, I believe, really fundamental. When we look into animal perceptions closely we find that even the most elementary of them show patterning—either spatial or temporal or both. Thus colour-constancy and figure-background relation, which Lorenz correctly implies to be gestalts, are of the very stuff of the innate releasive mechanism (i.r.m.) of animals, which he says are not gestalts. And since even olfactory and gustatory stimuli are temporal patterns, it seems that even these, contrary to Lorenz's contention, are of a gestalt quality. Now if Lorenz was right in his assumption that the innate releasive mechanism has no gestalt quality he would cut the ground from under his own feet, for he then proceeds, on the assumption that these very constancy factors are the evidence of a subconscious or unconscious gestalt-forming ' computor,' to elaborate a brilliantly original concept of the nature of intuition as simply the fullest development of the computor ; and he proceeds to point out how the type of mind which has this type of computing mechanism highly developed is the only one which can really excel at inductive research. Lorenz thus arrives at what he regards as an explanation of intuition which gives hope of ultimately turning out to be a physiological explanation. How different a conclusion to that of Huant (see above), quoted with approval by Dalcq. Yet as if sensing the philosophical predicament engendered by this assumption of physiological mechanism for ' knowing ' the external world, he closes with a brief mention of the ' reverential awe ' in which the research worker holds all Nature and he quotes Rudyard Kipling's Puran Bhagat, ' Nothing was farther from his mind than miracles. He believed that all things were one big miracle, and once a man has got to know that, he has got something to go upon.' Perhaps ethologist-physiologist Lorenz and embryologist-organicist Dalcq are not so very far apart after all ! Perhaps they are really both Vitalists !

Grey Walter, in considering the significance of alpha rhythms, leads us a step further towards the idea of a neural mechanism for the perception of gestalts, for that power to imagine pattern where there is confusion and to create cosmos where there was chaos. Arnheim follows appropriately with his study of gestalt psychology and artistic form, and his notion of the visual apparatus not as a passive photographic camera, but as a living entity coping with incoming images in active struggle, imparting order or form into relative chaos. Finally Gombrich, riding an artistic hobby horse, comes in at the finish with a ' substitutional ' or ' representational ' theory of the origin and evolution of artistic form—representation being originally the creation of effective substitutes out of a given material.

It is all very exciting and very stimulating. At times—as if in a nitrous oxide dream—we seem to be on the verge of discovering some great unifying principle, some final revelation of the unity of the cosmos. Then suddenly we are empiricist philosophers once more. Perhaps we are after all back at the perennial paradox of the theologian and epistemologist, forcing our own images on to the world of sense data, making God in our own image and yet *knowing* that in spite of our image making, our gestalts do in some way bridge the gap from seen to unseen ; do in some way give us some real knowledge of God and nature. Has not Coleridge said the last word ?

> So shalt thou see and hear
> The lovely shapes and sounds intelligible
> Of that eternal language which thy God
> Utters, who from eternity doth teach
> Himself in all, and all things in Himself.

W. H. THORPE

Nicolaus of Autrecourt. A Study in 14th Century Thought, Julius Rudolph Weinberg, Princeton University Press (London : Geoffrey Cumberlege), 1948. Pp. x + 242. 24s.

THE attention of English readers was drawn to this fourteenth-century French philosopher and logician by Hastings Rashdall in a paper entitled ' Nicholas de Ultricuria, a Medieval Hume,' published in the *Proceedings of the Aristotelian Society* in 1907. A year later J. Lappe published a detailed study of Nicholas' life and thought, with texts of his known writings, in the series, *Beiträge zur Geschichte der Philosophie des Mittelalters.* Subsequently Messrs Michalski, Gilson, Vignaux, Moody and others discussed different aspects of Nicholas' ideas (see p. 239 of this book). In 1939

Fr J. R. O'Donnell published in *Mediaeval Studies* the newly recovered text of the so-called *Exigit ordo executionis*, the longest and most important of Nicholas' writings, and in 1942 he published the most detailed historical study of Nicholas' philosophy since that by Lappe. Mr Weinberg's book is a philosophical study using the published texts for a purpose clearly indicated in the opening paragraph : ' We are often told that Truth never dies. . . . It is certain, however, that error is capable of repeated resurrection. Only thus can the recent attempt to revive Aristotelian epistemology and metaphysics be described. For the claims that Aristotelian philosophy made for natural knowledge are subjected to annihilating criticism in the fourteenth century by Nicolaus of Autrecourt and in the eighteenth century by David Hume . . . it is important for us to realise that a successful refutation of Aristotle occurred in the scholastic period itself.'

It is important for Mr Weinberg's readers to realise that the background to Nicholas' intellectual adventures was not some generally accepted body of philosophical doctrine constructed by ' the vain attempt of the scholastics to discover the truths of reason and the secrets of nature from the study of Aristotle and his Commentator, Averroes ' (p. 8). Active and original Western philosophy began in the late eleventh and early twelfth centuries with figures like the ' nominalist ' Roscelinus and Abelard, when the only writings of Aristotle known were the *Categories* and *De Interpretatione*. As further books, both by Aristotle and by the other thinkers whose writings from the body of Greek philosophy and science that has come down to us, were translated into Latin in the twelfth and early thirteenth centuries, they were incorporated into a living tradition as points of departure for original experimental, mathematical and philosophical research. No more than the Greek philosophy and science which was its nursing mother, or the modern philosophy and science to which in turn it gave rise, was scholastic philosophy and science uniform or static. The period in which Nicholas wrote was as diverse and rich in original ideas as any in the history of philosophy. Of a generation including Ockham and Buridan he was merely one of the most original of critics ; his attempts at construction were distinctly poorer than those by several of his contemporaries.

Nicholas' purpose was to discover the most certain possible foundation for knowledge, his point of departure being Aristotle's epistemology and metaphysics. Mr Weinberg takes us through the whole gamut of his critique, beginning with Nicholas' ' theory of evidence ', according to which the sources of certainty were limited to ' intuitive experience,' that is knowledge gained through the sense organs and through introspection, and to self-evident propositions and their necessary consequences. The second source of certainty Nicholas tried to reduce to the certainty of the law of noncontradiction (the ' first principle ') ; and the certainty of immediate knowledge of the objects of the outer and inner senses he tried to show was also guaranteed by the same law. Logically necessary implication involved an identity of premisses and conclusion ; simple cognitions involved an analogous identity of the direct act of cognising an object (usually external to the cognising person) and the reflex act of cognising that direct act. The certainty of a cognition, Nicholas held, did not provide evidence for the existence of anything beyond that cognition. 'From the fact that one thing is known to exist, it cannot be evidently inferred (by means of evidence that can be reduced to the first principle or to the certitude of the first principle) that another thing exists' (pp. 31, 32). From this conclusion he argued that it was impossible to have evident knowledge of the existence of causes or of substances external to the mind. Mr Weinberg goes on to show how Nicholas dealt with the problem whether *probable* knowledge of causes and substances was possible. There are interesting chapters on the meaning of 'probable' in the fourteenth century and on Nicholas' use of probable arguments to elaborate his positive views about the world and about human cognition.

For readers of this *Journal* the most interesting of Nicholas' theories about the world will probably be those about relations, *continua* and the vacuum, and about motion and change. On fourteenth-century treatment of these problems a considerable volume of research has been published since the pioneer writings of Duhem. Doubtless considerations of space led Mr Weinberg to keep his discussion of this work as brief as possible, but the result is far from satisfactory : detailed criticism of this aspect of his book has been made by Mr Marshall Clagett in *Isis*, 1949, **40**, 267–68. Since Mr Clagett's review further important results have been published by Miss Anneliese Maier in *Die Vorlaeufer Galileis im* 14. *Jahrhundert* (Rome, 1949). Nicholas followed Ockham in treating relations as concepts and in trying to eliminate them from the realm of being outside the mind. This view led him to assert the atomic nature of space and of time. Change was the aggregation and disaggregation of persisting, infrasensible parts. Motion, which Aristotle had held to be a process by which real attributes were generated and corrupted, Nicholas treated as nothing more than a set of propositions about a given body. As Mr Weinberg puts it, '" x is moved " means " x is at a at time t, x is separated from point b at time t, x is at b at time t_1 and separated from a "' (p. 168). The reader should compare this with the statements in Fr Philotheus Boehner's recent edition of *The Tractatus de Successivis attributed to William of Ockham* (*Franciscan Institute Publications*, No. 1, New York, 1944, pp. 45–46, 121–122).

The concluding chapters of Mr Weinberg's book deal with Nicholas' theory of cognition and his account of the objects of cognition. These chapters are well done. Taken as a whole, Mr Weinberg has made a useful invasion into fourteenth-century thought. The defects of the book, apart from a rather clumsy style which often makes his exposition hard to follow, are mostly the result of an insufficiently clear distinction between the historical account of Nicholas' views and the philosophical evaluation of these in relation to Mr Weinberg's own views.

A. C. Crombie

From Euclid to Eddington. The Tarner Lectures, 1947, Sir Edmund Whittaker, F.R.S., Cambridge University Press, 1949. Pp. ix + 212. 15s.

The lectures have as subject the historical development of physical theories. As the title indicates, the methodological approach is from the geometrisation of physics which is due to general relativity. The discussion of space, time, and motion with which the book begins is directed towards elucidating the geometry of the universe. Against this background of cosmology the concepts of classical and of quantum physics are introduced, and the book ends with a chapter on the Eddingtonian universe. The evolution of physics is presented in a very suggestive manner ; though on a high technical level, the lectures will bring enjoyment and information to both the layman and the physicist.

If any criticism can be made at all, it certainly does not concern any inaccuracies about facts ; rather it is the general approach that may be questioned. It may mislead the reader into believing that cosmology is as safe and sound as any other theory of physics ; but it is full of logical pitfalls. One cannot speak about the universe as one can speak about an isolated system—if ' universe ' designates all there is. What logical meaning can be given to a phrase like ' the beginning and end of the universe ' ? Similarly, ' the direction and the zero-point of time ' are logically obscure ideas, the more so when the concept of time is applied to the whole universe; for time is not like a river which has a direction and a source and which never stops flowing (though it ends in the sea).

The theory of knowledge inherited from classical philosophy is not adequate for modern physics. Eddington's view which is put forward in these lectures is based on this epistemology : there is the search for an ultimate reality and for a universal principle. But the more comprehensive a principle, the more logically empty it is ; an all-embracing principle would say exactly nothing about the world. The fine-structure constant does not reveal an inner harmony in nature though it may suggest a re-formulation of microscopic theory in terms, say, of a quantum of length. The Laplacean dream of a world-formula belongs to a bygone age.

There is no need to introduce principles of impotence into physics ; for negative existential statements are logically equivalent to universal implications which are the standard form of scientific law. The difference is merely pragmatic and psychological, whether we express the laws of thermodynamics in one or the other formulation.

Speculation may be, psychologically, a help in discovery. But scientific theory must lead to new, and testable, results, and this is possible only if the basic concepts are sufficiently clear. Cosmology, and fundamental theory, leave much to be desired in this respect. The case in its favour is persuasively and entertainingly argued in these Tarner lectures.

E. H. Hutten

Kleiner Grundriss der Naturphilosophie, Eduard May, Westkulturverlag Anton Hain, Meisenheim-am-Glan, 1949. Pp. 106. DM. 4.90.

The author explains that ' Naturphilosophie,' as he uses the term, is not a discipline lying between science and philosophy seeking to unite the natural sciences, but a branch of philosophy which attempts to account for the existence of the sciences by going beneath them to problems outside the scope of scientific methods. This brief and inevitably somewhat superficial survey of a vast field appears to be intended as an introduction for students. Those interested in German philosophical thought and in scientific method may find the book useful for references to problems ranging from logic and classification to physics, gestalt, and evolution, though it cannot be regarded as comprehensive or up-to-date, even on German work in the fields covered.

L. L. W

Histoire de la Mécanique, René Dugas, Éditions de Griffon, Neuchatel, 1950. Pp. 650. Swiss Francs 65. (Agent for Great Britain : Félix Rose, 54 Blenheim Terrace, London, N.W. 8.)

This very important book by a professor at the École Polytechnique in Paris is the most complete history of mechanics yet published and is likely to become the standard work on the subject. It has rightly been given a notable distinction by the French Académie des Sciences. A vast amount of information, much of it based directly on a study of original sources, is reduced to order and expounded in an admirably clear and simple style. There is a preface by Louis de Broglie.

The plan of the book is to give a chronological account of the development of general principles, interpreting the scope of the mechanics strictly and excluding, for example, all but the strictly mechanical aspects of astronomy. Hydrodynamics is also excluded from detailed consideration.

The history of mechanics is divided into five parts, the first four being given roughly the same number of pages and the fifth nearly twice that number. A good mathematical equipment is assumed throughout, the demands on the reader naturally increasing in the later parts of the book. The first part, ' Les Précurseurs ', covers the period from Aristotle to Kepler ; the second is devoted to the foundation of classical mechanics in the seventeenth century ; the third describes the organisation and development of the principles of classical mechanics in the eighteenth century ; the fourth discusses the evolutionary tendencies in classical mechanics after Lagrange ; the fifth gives an account of the principles of modern mechanics : special and general relativity, wave mechanics and quantum mechanics.

Such a book naturally calls for comparison with Mach's *Science of Mechanics* ; in fact it is complementary to Mach's classic. Mach's approach was philosophical, Professor Dugas' is historical. Mach's intention was to make critical use of history to expose the origins of certain problems in contemporary mechanics ; he was able to organise his material to a high degree systematically because he saw it all from the point of view of a particular school of physics and of philosophical positivism. Professor Dugas is concerned not to discover the development of a point of view, but to describe the problems of mechanics as they arose historically for the men who first formulated and attempted to solve them. The result is less systematic than Mach's, but more complete and more satisfactory.

Though the last part of the book is the most developed and will perhaps be the part most used by physicists, the first two parts are in many ways the most interesting for the general scientific reader with philosophical inclinations. ' La méchanique est une des branches de la physique dont le bagage de principles est à la fois le plus restreint en volume et le plus riche de conséquences utiles. Il est, d'autre part, peu de sciences qui aient exigé plus d'efforts de l'esprit humain : la conquête de quelques axiomes a duré plus de deux mille ans.' The Greeks invented statics and brought this science to great perfection, but their understanding of dynamics remained very primitive : it is astonishing how apparently simple are the principles the grasp of which made the development of seventeenth-century dynamics possible. For nearly two thousand years the dynamical ideas of Greeks, Arabs and Western Europeans were dominated by Aristotle's conception of motion as a process requiring a continuous efficient cause to maintain it : velocity was the result of two external ' forces,' a ' force ' producing motion, and resistance from the medium. That these conceptions did not enable certain specific problems, the motion of freely falling bodies and of projectiles, to be correctly formulated was appreciated by the Greeks themselves. Some late Greek and Arab writers made the first moves towards new conceptions, and these were taken up again and developed in the West in the thirteenth and fourteenth centuries. In the fourteenth century, William of Ockham attacked the whole idea of motion as a process and said that scientific theories were concerned only with descriptions of the relations between observable objects. Jean Buridan in Paris, probably inspired by mathematical work at Oxford, gave precise formulation to the theory of *impetus*, his definition of *impetus* being similar to the seventeenth-century definition of momentum (Galileo used the words *impeto* and *momento* as synonyms) ; Buridan said also that a body in motion acted on by no ' force ' or resistance would continue with uniform velocity indefinitely and that ' force ' produced a *change* of velocity. In Oxford, Thomas Bradwardine and his successors at Merton College began to develop mathematical functions, defined acceleration as the velocity of a velocity, formulated and proved the kinematical theorem to describe the motion of a body moving with uniform acceleration. This fourteenth-century work, often intuitive and restricted by a limited knowledge of mathematics, is the beginning of the formulation of seventeenth-century dynamics ; the succeeding steps towards the seventeenth-century conceptions of inertia and mass were infinitely difficult for those who took them, involving a most complex combination of philosophical criticism and mathematical and experimental technique. That the result appears so simple to us is the greatest compliment we could pay to the genius of Bradwardine and Buridan, of Galileo and Newton.

For his account of mechanics before Galileo, Professor Dugas relies almost entirely upon Pierre Duhem. It is of great value to have a concise account of the results of historical research published in Duhem's voluminous writings. Professor Dugas' account of the history of mechanics in the middle ages is the fullest yet published in a general book. The last of Duhem's writings to be published appeared in 1917, and since then a considerable volume of further work on mechanics from the thirteenth to the sixteenth century has appeared in print. Some of Duhem's results have inevitably been criticised, though the bulk remains intact. It is to be hoped that in later editions of his book Professor Dugas will take account of more recent work, for example that of S. Pines on Arab precursors of *impetus*, and that of Anneliese Maier, E. J. Dijksterhuis, Marshall Clagett, E. A. Moody, Alexandre Koyré and C. B. Boyer on dynamics and mathematical functions from the fourteenth-century down to the time of Galileo and Descartes. The ideas of, and relations between Ockham, Buridan, Oresme and other fourteenth-century writers have been considerably clarified by this work. Moreover, some of the most important texts have been published in modern editions, whereas Professor Dugas quotes mainly from Duhem's translations. Some small points : the date of John Philoponus of Alexandria is A.D. sixth, not seventh century (p. 47), of William of Ockham, *c.* 1300–1349, not as given on page 48 ; the name Richard Swineshead probably covers three different individuals but ' Ghlymi Eshedi ' (p. 66) is not the name of one of them : it is due to a copyist's mistake not detected by Duhem (see Lynn Thorndike, *History of Magic and Experimental Science*, 1934, III, 373-4).

Taking into account the whole of Professor Dugas' book, the range of his treatment covering the history of mechanics from the time of Aristotle to the mid-twentieth century, these points of criticism are small indeed. His book is very illuminating on a multitude of subjects ; it can look forward to a long and useful life.

A. C. Crombie

Études de Philosophie des Sciences, en hommage à Ferdinand Gonseth, à l'occasion de son soixantième anniversaire, Série Dialectica, Bibliothèque Scientifique fasc. 20, Neuchâtel 1950. Pp. 174.

Bachelard, Gaston	L'idonéisme ou l'exactitude discursive
Bernays, Paul	Mathematische Existenz und Widerspruchs-Freiheit
Besso, Michel-A.	Réciprocité entre objet et propriété
Bouligand, Georges	L'abandon de la tendance prédicative en mathématiques
Clerc, Charly	Quand la dialectique se fait homme
Destouches, Jean-Louis	Les thèses fondamentales de l'idonéisme
Destouches-Février, Paulette	Observateur et prévisions en physique théorique
Dupréel, Eugène	Similitude et compatibilité
Eckmann, Beno	Continu et discontinu
Fiala, Félix	Réflexions sur la métaphysique du calcul formel
Gagnebin, S.	La mathématique universelle d'après Edmund Husserl
Hainard, Robert	Contact avec la réalité
König, H.	Zum Thema : Technische Nomenklatur und Erkenntniskritik
Nolfi, P.	Der Beitrag der Mathematik am sozialen Fortschritt
Perelman, Ch.	La quête du rationnel
Piaget, Jean	Schémas mathématiques, biologiques et physiques
Pólya, G.	Let us Teach Guessing
Rossel, Jean	Quelques aspects épistémologiques d'un demi-siècle de physique
	Publications de Ferdinand Gonseth

This table of contents gives evidence of Gonseth's influence in Switzerland and the French-speaking countries and of the variety of his interests.

Bernays's contribution is of particular interest in view of recent controversies on the ontological status of mathematical entities in which, among others, R. Carnap, H. Feigl, W. V. Quine, and G. Ryle have taken part. Bernays stresses the relatively unproblematic character of ' bezogene Existenz ' (existence within an ideal framework, given beforehand) and points out that, when we state or prove the consistency of a set of postulates to constitute a sufficient condition for the existence of a model, only ' bezogene Existenz ' is involved. The existence of the corresponding framework cannot be ascertained in this manner. This brings Bernays to consider the situation of research on the foundations of mathematics as similar, in a sense, to that of the theories in physical science. Destouches has given his discussion of a number of favourite themes of Gonseth's ' la forme dialoguée chère au père de l'idonéisme.' Eckmann's all too concise contribution gives an interesting sketch of the interpenetration of algebraic and topological methods in modern mathematics. The readers of this *Journal* will find in the other contributions also many interesting observations.

E. W. Beth

The Life of Science, George Sarton, Henry Schuman, New York, 1948. Pp. 197. $3.50.

This book, which is described as the ' keynote volume ' in the ' Life of Science ' series, consists of a collection of Dr Sarton's papers on various occasions. The dates of the essays vary from 1916 to 1941 but most of them were written in the earlier part of this period. In consequence their mood is sanguine to a degree unusual today, even a little incongruous ; but they reflect vividly the sincerity, energy, and vision of the world's senior historian of science.

Part I consists of three essays on the history of science in general. The first two are rather slight. The third, written in 1916, urges that the history of science should be approached in the context of general history—that the development of science should be considered in relation to the intellectual exchanges and other influences brought into play by other elements in civilisation, such as technology, religion, art. The aim is a closer contact between scientist and philosopher, to achieve a new synthesis, a closer unity of knowledge (p. 30) and a new humanism, for which Dr Sarton expresses great hopes. ' The history of science,' he writes, ' if it is understood in a really philosophic way, will broaden our horizon and sympathy ; it will raise our intellectual and moral standards ; it will deepen our comprehension of man and nature.' In Part II, Dr Sarton reprints essays on four representatives of scientific life : Leonardo da Vinci, Evariste Galois, Ernest Renan, and Herbert Spencer. These sketches are, as we should expect, lively, warm-hearted, and of wide sympathies ; perhaps it is significant of Dr Sarton's preferences that each figure has at least a touch of the rebel. A long essay on ' East and West in the history of science ' (Part III), forms perhaps the most valuable part of the book, for here Dr Sarton's immense and comprehensive learning in the literature of science, his humanist principles and desire for synthesis, combine to produce a fascinating survey. Finally the author urges the establishment of an institute for the study of the history of science, a plea that reminds us how much he has done, especially as editor of *Isis*, with its critical biographies and editorial prefaces, for the better organisation of these studies. The general reader as well as the scientist will enjoy this vigorous and wide-ranging series of essays.

It remains to ask whether the picture of science that Dr Sarton has here painted is a good portrait. It represents science as an intellectual adventure, as forward-looking, progressive and cumulative ; as an absorbing and exciting pursuit, capable of engaging the most acute minds ; as contributing to the unity of mankind, and as an important element in social change. This no doubt is admirable ; but it is not all that Dr Sarton holds. He urges that science should be regarded as the whole of objective and verifiable knowledge, and that scientific method is the ' basis of well-nigh all our knowledge '. It is curious that he gives no account of historical knowledge, which is not obtained by the inductive method. His omission of philosophy and theology from the field of knowledge is less unusual, in view of the attacks of some contemporary philosophers on metaphysics and ethics, but to some of us their criticisms seem too radical and, as Professor Barnes noted recently, the tide of battle may have begun to flow the other way. If this continues, so that some kind of significance is once more conceded to constructive philosophy, Dr Sarton's rejection of the non-inductive disciplines will seem less plausible. A genuine synthesis of knowledge would seem to demand a comparative study of rational methods and of the kinds of conclusion reached by each of them, rather than an *a priori* rejection of all methods other than that of the natural sciences. Dr Sarton urges the necessity of non-scientific experience, of art and religion and a philosophical approach ; he omits the corollary, the existence of non-scientific types of knowledge.

E. F. Caldin

Reflections of a Physicist, P. W. Bridgman, Philosophical Library Inc., New York, 1950. Pp. xii + 392 $5.00.

The book is a collection of popular essays previously published in various journals during the last twenty years. It is divided into five groups of which only one deals with scientific topics.

The operationalist view can be seen here to arise from Dewey's instrumentalism (itself a product of pragmatism) and from Wittgenstein's *Tractatus*. The meaning of a statement is given by the physical or mental (paper-and-pencil) operations by which it is verified. The merit of operationalism lies in its emphasis on scientific practice ; but, clearly, the analysis it offers does not suffice. For the distinction of physical and mental operations is obscure ; and what may we accept as operation when even hypothetical operations must be admitted ? Another weakness of this approach is its extreme solipsism, e.g. science is said to be essentially private. This shows that operationalism is an epistemology confined within the classical tradition, with the typical confusion of logical and psychological issues. No philosophical scheme, no -ism, will ever be able to account for actual science.

The critical attitude of operationalism is often useful. The discussion pointing out the limitations of present-day concepts, e.g. in thermodynamics, is stimulating. The application of the Uncertainty relations to the motion of stars is, however, highly doubtful.

The greater part of the book is concerned with social and political problems ; these are, I am afraid, not always as simple as they are made out to be. Scientists nowadays are prone to regard themselves as experts in every field of human activity because they may have knowledge in one particular field; and they behave as if they were the innocent victims of power politics. But the scientists, too, have become politicians : in public, they proclaim the freedom of research while, privately, they reap the rewards of secret Government employment. Professor Bridgman rightly argues that, then, they cannot claim to be disinterested and demand special privileges.

The book is mainly addressed to the layman ; but its level-headedness, and the refreshingly unpretentious manner of exposition, will also appeal to the scientist.

E. H. Hutten

Principles of Mathematical Logic, D. Hilbert and W. Ackermann. (Translated by L. M. Hammond, G. G. Leckie and F. Steinhardt. Edited and with notes by R. E. Luce), Chelsea Publishing Co., New York, 1950. Pp. xii + 172. $3.50.

This is a translation of the very well-known *Grundzüge der theoretischen Logik* of Hilbert and Ackermann, one of the classic texts of mathematical logic. The translators, the editor and the publishers deserve to be thanked for making it available. It is made from the second German edition (1938) and it should be mentioned that a third German edition has been published this year (1951). The editor indicates in his preface the minor changes from the original introduced into the English edition, as well as two corrections to the argument depending on the work of A. Church and one depending on that of W. V. Quine. Also he has added a few notes, mainly about usages concerned with certain standard terms, and a short bibliography.

The scope of the work need be only briefly recalled. Chapter I is on the 'Sentential Calculus' (propositional calculus) and includes, naturally, the formulation of the system of axioms and a discussion of its consistency, independence and completeness. The short Chapter II on the 'Calculus of Classes' contains a systematic theory of traditional logical inferences. Chapter III on the 'Restricted Predicate Calculus' (functional calculus) is the core of the work. The propositional symbols of Chapter I are reinterpreted as predicate-symbols in Chapter II and these become functional symbols in Chapter III, the subjects of the predicates appearing as the arguments of the functions, and the notation is completed by introducing universal and existential 'quantifiers' for these arguments. The calculus requires the addition of two axioms to the system given in Chapter I and this requires a fresh discussion of consistency, independence and completeness; that of completeness (in a restricted sense) follows the work of K. Gödel. The chapter closes with a discussion of the 'decision problem.' Chapter IV gives an account of the 'Extended Predicate Calculus.' Here a predicate of the 'second level' includes among its arguments a predicate (of the 'first level') of the sort already treated, and so on to any higher level. This procedure, introduced by Whitehead and Russell, yields the calculus appropriate for expressing the processes of inference employed in mathematical analysis.

For over twenty years, this has deservedly been a standard work on the subject. The prospect of its remaining such for many years to come has been enhanced by its now being made available in the present excellent translation to a still wider class of readers.

W. H. McCrea

Science: Sense and Nonsense, J. L. Synge, Jonathan Cape Ltd., London, 1951. Pp. 156. 8s. 6d.

There is only one adjective that I really must use to describe this book—it is delicious. Professor Synge writes from the Institute of Advanced Studies at Dublin and he tells us that this little *jeu d'esprit* is based on a course of statutory lectures at that Institute. If so, they must have been unlike any other course that I have ever known, reminding me of C. S. Lewis' *Screwtape Letters* for the delicacy with which they prick familiar bubbles. The blurb on the cover is probably right in saying that some of the contents, such as the tale of the Sea Captain's Box (elucidating the propriety with which we may or may not use the symbol $\sqrt{2}$ in the mathematical world and in the physical world), and the Socratic Dialogue on Relativity, are likely to become classics of exposition.

Most of us who read this *Journal* know well enough the shaky character of science and the impossibility of defining even the categories we use. But few of us will not discover in the whimsicalities of this book something that is fresh. And we shall all relish the *bon-mots* with which it abounds and which serve, as Synge himself says, to keep us awake even while discussing the philosophy of science. Thus, on the utility function of science —' they would say it in Russia, they would think it in the United States.' Or, on the inescapable need for some sort of theory, ' measurement without imagination is only an empty sieve.'

It is impossible to do justice to this book in a review, or even to summarise it. ' Mathematics for the Million,' according to the author, will be quite impossible until people really think as logically as the Greeks ; ' Science for the Million ' will have to wait until they have been initiated. For there is much in science that only makes sense to those inside it—others must simply worship, since they cannot understand. Yet science is not wholly hieratic, it is partly Art : yet not only Art, for it progresses, and as Whistler said ' Art cannot progress : what begins with the Infinite cannot go further.' As Synge says : ' Science is neither wholly Church nor wholly theatre.'

It is hard to cavil at such a jolly book, but I would have liked a little more on the way in which a *modus vivendi* (or synthesis, or what-you-will) is achieved within the individual scientist, since, as we are told, he is compounded out of ' aesthetic, mystic, practical man and craftsman.' They do cohere, for many of the scientists we know are the most balanced of all men. And some at least of these would have said that their parts cohere in the idea of God. I should also have liked to know more about some of the compelling principles of science, such as the idea of simplicity, or Occam's razor. These seem to me to lead us into other regions of reality (elusive word) than those with which this book is concerned. But no bag is big enough to hold everything, and this is grand value for 8s. 6d.

C. A. COULSON

The Rise of Scientific Philosophy, Hans Reichenbach, University of California Press, Berkeley and Los Angeles (London : Cambridge University Press), 1951. Pp. xi. + 333. 28s.

THE rise of scientific philosophy as traced by Professor Reichenbach makes very interesting and easy reading. One is impressed by the breadth of his subject and therefore any attempt to review it must start with some remarks on the character of the review.

First, the reviewer thinks along the lines that Professor Reichenbach discusses and therefore welcomes an account from so eminent a philosopher and physicist. Secondly, if the reviewer did not think along these lines but was a 'speculative philosopher' no doubt the review would be more detailed and critical. I am sure there would be plenty of scope for disagreement. Thirdly, it appears from the style of the book that it is not intended to be a deep account of some new philosophical thesis, but rather a readable, reasoned account and criticism of older philosophical methods. In this way it stresses that philosophy is a study of problems and not a study of answers, and therefore stimulates the layman to take an interest in philosophical problems which have arisen as a result of recent scientific work. Whether its author intended it or not, the book has a missionary flavour. To those who wonder why philosophy now bothers about logical analysis, and who want to know what this entails, this book will provide the answer. I do not suggest that it is a new gospel of truth in all respects. One cannot be a missionary and expound faultlessly a new philosophy without a certain amount of laxity in the popularisation. Therefore, there are places where one is not entirely happy, but the overall effect of the work is of a neat account of the general tenets of logical empiricism.

Professor Reichenbach divides his book into two parts. The first part is an account of the aims of speculative philosophy. He briefly describes the work of Plato, Aristotle, Spinoza, Kant on the speculative side, and the medieval empiricists, Bacon, Locke and Hume on the empirical side. He objects to speculative philosophy because it attempts to give answers when there was no reliable method (science) for doing so. This aim of answering all problems was based upon the urge for certainty in knowledge. Certainty could be obtained only by searching for generality in the world and then offering the explanations which were merely the satisfaction of psychological desires. Such explanations are frequently found to consist of *naive* parallelisms or analogies, and hence they are regarded as pseudo-explanations. Reichenbach points out that psychological satisfaction is not the only aim of explanation. He suggests that pseudo-explanations fail because too easily they can involve concepts which are not translatable into verifiable experiences. Because of this the theories which incorporated them lacked predictive power. I think that in laying this charge too heavily against the early philosophers, Professor Reichenbach is less than fair to them. One has to be careful not to say that because we have no key to open a door it is incorrect to use a jemmy. In the scientific world, the key of today may turn out to be the jemmy of tomorrow.

Each part of the considerations given to the speculative philosophers ends with an account of Kant. There are also some interesting remarks in defence of Hume and the uselessness, for practical purposes, of statements about the existence of entities, on page 111.

All the speculative philosophers erred, according to Professor

Reichenbach by giving to reason too great a power in the search for knowledge, particularly when reason was unaided by observation.

In the second part of the book the empirical approach is shown to be the basis for the scientific philosophy. The problems posed by scientific philosophy are solved 'not through vague generalisations or picturesque descriptions of the relation between man and the world, but through technical work.' In the course of this work the scientist 'found answers unheard of in the history of philosophy.' The rest of the book gives the scientific answers to philosophical questions and notes the importance in this generation of concentrating on philosophical analysis. The professional philosopher of science is one who concentrates on logical analysis 'aiming at clarification rather than discovery.'

One is unfavourably impressed by three things as one reads the account of this new philosophy. The first is the unquestioning attitude to the truthfulness of logic and mathematics which is adopted all through the book. Is this not perhaps a new form of rationalism ? Can we not, in principle, question the validity of the methods of logical derivation. For example, we usually adopt a two-valued logic and even Reichenbach suggests that we might use a three-valued logic in certain connections. Surely it is not only knowledge that is defined by the use to which it is put but also our concepts of truth, and our methods of derivation are retained because they enable us to predict and to plan our experiments and lives accordingly. To present an unquestioning attitude to the validity of logic and mathematics is to take their truth values *a priori*, and this is what we are trying to avoid.

Secondly, while Professor Reichenbach's contributions to the theory and axiomatisation of probability are well known from his other works, it is not necessarily so important in scientific practice as he would lead us to believe. He adopts his point of view because he feels that it is the job of logic to justify theories. I am not persuaded that theories need more justification than believing in them and using them if, and only if, they lead to new consequences as well as the ones we hoped for.

Thirdly, I found the account of ethics disappointing. Reichenbach suggests that volitions, our individual volitions, are the basis for moral directives which we issue to each other. In the group to which we belong these volitions come into conflict with those of other people. He therefore proposes a democratic principle 'Everybody is entitled to set up his own moral imperatives and to demand that everyone follow these imperatives.' This is merely an invitation to take active part in the struggle of opinions. Its philosophical position is doubtful. By acting on our volitions we learn through error and our volitions are conditioned in the group by the learning of cognitive relations. I feel that this is an easy way out of all moral problems by the simple attitude of when in Rome do as the Romans do. One hopes that the Romans are morally right—but that is the very thing one is not justified in hoping according to Professor Reichenbach. It belongs to speculative philosophy !

Finally, it is a pity that in such a general book there is no mention of many people who have helped in the rise of scientific philosophy, and no bibliography to aid those who have been interested by the account and wish to go elsewhere for deeper study.

R. F. J. Withers

Einführung in die Logistik, vorzüglich in den Modalkalkül, Oskar Becker, Westkulturverlag Anton Hain, Meisenheim-am-Glan, 1951. Pp. 96. DM. 15.

After a brief historical introduction this exposition of logistic begins with Boolean algebra, which is treated in considerable detail. This is followed by applications, leading up to the sentential and functional calculuses. The last section, occupying twenty-five pages of the total, is devoted to modal logic. The bibliography contains the names of thirty-one writers, fourteen of whom are now dead. It does not contain the names of Church, Gödel, Quine, Łukasiewicz, or Tarski.

There are some printer's errors which will be puzzling for beginners. On p. 11 in the third Peano axiom a ' U ' is printed instead of an implication sign. On p. 45 ' *τἐίσ* ' is printed where ' *ἐστί* ' is intended. Two lines further on, the circumflex over ' x ' should be over the ' x ' next following. On pp. 67 and 68, lines 1 and 11 from bottom respectively, an inclusion sign is printed where an implication sign is intended. On p. 74, line 13 from bottom, ' ist kein " Kettenschluss " ' should read ' ist ein " Kettenschluss ".'

As a book for beginners this can hardly be recommended. The explanations are too much condensed. For example, dots are used as brackets, and on p. 43 ' vollständig ' is used in its technical sense, without explanation. The reference to the theory of types is much too brief for a beginner; and it is hardly correct to say that the *unverzweigte* theory was first used by Russell and Ramsey. On p. 45 there appears to be confusion between many-termed predicates and many-termed relations. The *Abtrennungsregel* is mentioned among the rules of inference, but nothing appears to be said about the rule of substitution. Thus the book is not an introduction in the sense in which Tarski's *Introduction to Logic* and Carnap's *Abriss der Logistik* are introductions. It will chiefly appeal to those who already have some knowledge of the subject and are especially interested in modal logic.

J. H. Woodger

Essays on Logic and Language, edited with an Introduction by Antony Flew, Basil Blackwell, Oxford, 1951. Pp. vii + 206. 16s.

Readers of this book are provided with material from which to discover what British philosophers at the present day are trying to do. They will find that, in spite of the title, although there is a lot here about language there is not so much about logic. They will also get the impression from the Introduction that modern British philosophers are somewhat insular in their outlook. For although the modern concentration on language may be said to owe its original inspiration to the German mathematical philosopher Frege, the present essayists, according to the Introduction, trace their intellectual descent to the British empiricist writers : Hobbes, Locke, Berkeley and Hume. It is true that in the same place the discovery ' which is most central and most fundamental . . . to the dominant tendency in modern British philosophising ' is attributed to Wittgenstein (although it is also recorded here that he himself credited it to his teacher Bertrand Russell). But even this might not have happened had not Wittgenstein been a pupil of Lord Russell or if he had not taught in England. Frege is only mentioned incidentally in a footnote and only in connection with an *English* translation of his *Grundlagen der Arithmetik*. Not one of the great names—Austrian, American, German, Italian or Polish—which occur in the gap between Frege and the present day is mentioned. The work of the immediate successors of Frege culminated in the publication of *Principia Mathematica*, but very great advances have been made since the appearance of the second edition of that celebrated work. Perhaps the chief credit, as the original source of most subsequent developments, should go to the Poles, especially to Leśniewski, Łukasiewicz and their gifted pupil Tarski. They were themselves pupils of the veteran Polish philosopher Twardowski who was among the first to urge the use of scientific methods in philosophy. After the liberation of their country in 1919 these men subjected *Principia Mathematica* to a minute analysis and filled in the gaps it had left open. Taking over the distinction which Hilbert had made between mathematics and metamathematics, they extended it to logic and metalogic and still further to language and metalanguage. They also made clear the distinction between the morphology of language and its semantics, and they laid the foundations of whole new sciences with these titles, which others have subsequently developed. They showed how some of the antinomies which have puzzled philosophers depended for their understanding upon the distinction between language and metalanguage and that some cannot be satisfactorily dealt with in a natural language at all.

Now either the present essayists are mistaken about the original source of the ideas with which they work or they ignore it. How otherwise are we to understand their apparent insularity ? But no one who seriously wishes to study the part played by language in science and philosophy can successfully neglect these sources indefinitely.

Not all the essays in this book are of interest to readers of this *Journal.* They range over the following topics, among others : time, induction, sense data, verifiability, law and political theory. Professor Ryle has contributed an article entitled *Systematically Misleading Expressions*, and he himself seems to fall (systematically) into one of the linguistic traps which he discusses. For among his systematically misleading expressions are what he calls ' the '-phrases, and throughout the essay the author uses the expression ' the logical form of the fact ' ; for example in such sentences as (p. 17) : ' There is a trap . . . which threatens the man who assumes that every statement gives in its syntax a clue to the logical form of the fact that it records.' He discusses, for example, ' Jones hates the thought of going to hospital ' and re-words it in such a way that the phrase ' the thought of . . .' disappears, so that ' For it to be true, the world must contain a Jones who is sometimes thinking and sometimes, say, sleeping ' ; but it need no more contain both Jones and ' the thought or idea of so and so ' than it need contain both someone called *Jones* and something called *Sleep*. Now presumably it would be all to the good if a similar procedure were applied to ' the logical form of the fact.' Is it really necessary, in this connection, to speak of facts at all ? Do they ' have ' something which can usefully be called their logical form ? Is it not only certain *linguistic expressions* which have a logical form, and is it not clear that although we can speak of the *grammatical* form of the linguistic expressions of everyday language (because there are rules of grammar) we can only speak of the logical forms of the various expressions in artificial languages into which we may care to translate them ? Because such languages have been so constructed that each meaningful expression in them does have a prescribed and specifiable logical form.

What seems to have happened is somewhat as follows. Logicians have devised certain artificial languages for scientific purposes, such as the language of the lower functional calculus. Well formed formulas in such a language can be said to have a logical form because rules are laid down in the metalanguage which determine which combinations of its signs are to be regarded as meaningful. In particular, they determine which combinations are to be called sentential functions, i.e. expressions which yield statements when all the dummy names which occur in them are replaced by genuine names. Now when people speak about the logical form of an expression in a natural language what they seem to have in mind is this : How would this expression be translated into the language of this or that calculus ? And it is clear that there are usually many ways of doing this even within the language of a single calculus, so that there is no such thing as *the* logical form of such an expression.

For example, suppose we are asked what is the logical form of the statement 'Tom is bald.' We can say that it has the form of 'Fx' in the language of the lower functional calculus, because we can replace 'F' by the one-place predicate 'is bald' and 'x' by 'Tom.' But we can equally well say that it has the form of '$x \epsilon \alpha$' in the language of the calculus of classes, because we can replace 'α' by 'bald' interpreted as a class-designation, 'x' by 'Tom' as before and we can interpret the sign of class-membership as a translation of 'is.' But that is by no means the end of the matter. We could set up a convention of the following kind: we could agree that 'x is bald' is to be an abbreviation for 'there is a y such that y is the head of x, and there is a class α such that every member of α is a hair and is part of y and the cardinal number of α is less than 100'. In accordance with this convention 'Tom is bald' would have quite a complicated logical form. Is it not therefore clear that there is no such thing as *the* logical form of this grammatically simple sentence? If we are to use the expression 'logical form of x' we must always relativise it to the particular calculus to the language of which x belongs. Which one we use will be determined by the scientific circumstances under which the problem arises. In non-scientific contexts such questions never seriously arise. For although Tom's mother may indignantly repudiate the suggestion that her precious son is bald, others may still be of that opinion, but there is no need to decide the matter.

Explicitly or implicitly Professor Ryle's essay seems to involve the following assumptions:

(1) Every fact has something called its logical form.
(2) Every statement has a syntactical form.
(3) Every true statement records a fact.
(4) The syntactical form of some statements is *improper* to the logical form of the facts recorded. Such statements are or contain misleading expressions.
(5) In order to discover whether a statement is systematically misleading we first determine its syntactical form, then the logical form of the fact recorded, then we compare them to see whether the one is proper to the other.

But Professor Ryle gives no explicit instructions about how these last operations are to be performed or about the criteria of propriety. Indeed on page 34 he himself raises pertinent doubts about the whole procedure. Dr Waismann, in his article entitled *Verifiability* goes even further. On page 137 he writes: 'People are inclined to think that there is a world of facts as opposed to a world of words which describe these facts. I am not too happy about that.' On page 139 he gives examples 'which tend to show that what is called a fact depends on the linguistic medium through which we see it.' It would seem from all this that 'fact' is a strong candidate for inclusion among the misleading expressions.

There is also a mistake in the article entitled *The Philosopher's Use of Analogy* by Margaret Macdonald, which may be misleading. On page 89 it is stated that 'All horses neigh' means 'Whatever is equine neighs' or 'x is equine and x neighs' is always true. Now saying '"x is equine and x neighs" is always true' is one way of saying: 'for all x, x is equine and x neighs.' But this is certainly *not* a translation of 'All horses neigh' but of 'Everything is a horse and neighs' which is quite a different matter. If we want to translate 'All horses neigh' into a language which uses sentential functions we must write: 'for all x, if x is equine then x neighs.'

By far the most interesting essay from the point of view of the philosophy of science is the one by Dr Waismann which has already been referred to. Most people will have no hesitation in accepting what the author says about the difficulty of verifying empirical statements on account of what he calls the 'open texture' of empirical concepts. But nowadays people speak not of verifiability and verification, but rather of confirmability and confirmation, and this makes a more modest claim. If we adopt this substitution the sting will be taken out of some of Dr Waismann's criticisms. But there is one point in the discussion which seems to be left rather obscure. Perhaps it may be put in the following way. Suppose we have a 'law' to the effect that all cats have nine lives. Suppose we also adopt the statement that Felix is a cat. Now either intuitively or according to laws commonly adopted in logical calculuses it would be said that the statement: 'Felix has nine lives' follows from the above two statements, the law and the 'boundary condition.' We now proceed to test the law by counting Felix's lives. If this proves to be nine we can say that so far as this single test is concerned the law is confirmed. But if Felix fails to revive after his eighth death, or if he revives after his ninth, we seem to have various alternatives. We can say that the law is falsified and reject it, or, if it has been a useful law and we want to keep it, we can say that we have counted wrongly or that owing to the open texture of 'cat' we were mistaken about Felix. He was not a cat after all, but some strange rare monster masquerading as a cat. Even if this fails we can still preserve our law by assuming that there was something unusual about the environment in which Felix lived which will explain why he did not conform to the law. In practice we choose the alternative for which we have the best confirmatory data. We retain a law so long as it continues to be useful and so long as no one has proposed a better one. Now what Dr Waismann seems to suggest is that the situation would be in some way improved if we altered the *logical* law by which we infer 'Felix has nine lives' from the two premisses. But he offers no suggestion regarding the form such alterations should take, nor is it explained how they would help. But it is a little difficult to discuss this essay because it was written in reply to a previous paper in a symposium, and the precise point at issue between the symposiasts is not explained. For that reason the above may be a gross misrepresentation of what Dr Waismann is saying. It is a pity that this essay was not rewritten before being included in the present volume.

J. H. Woodger

Foundations of Analysis (The Arithmetic of Whole, Rational, Irrational and Complex Numbers), Edmund Landau. (Translated by F. Steinhardt), Chelsea Publishing Co., New York, 1951. Pp. xiv + 134. $3.25.

This is a famous little book by one of the most distinguished pure mathematicians of recent times. It has been so well known since its first appearance in 1930 that a review at the present time need scarcely do more than welcome its translation into English. Indeed, at any time a reviewer could have done little else than say in less colourful language what the author himself has said in his two highly entertaining prefaces, one for the student and one for the teacher. For the sake of these prefaces alone, a translation is justified. The rest of the book could have been read by anyone with almost no knowledge of German but possessing a certain amount of initiative. Unfortunately, it was not easy to persuade students that such was the case, and it is a cause of much satisfaction that the work is now available to them in English. (It is to be recalled that in 1946 the same publishers produced a reprint of the German text with English versions of the prefaces by the same translator and with a German-English vocabulary.)

The arithmetisation of mathematical analysis consists in making it depend solely upon the natural numbers. All that is assumed in this book is the existence of these numbers, that is to say a set of objects whose properties are entirely described by the five axioms of Peano. The problem of the definition of 'number' in any still more fundamental sense is not involved. The sequence is then :—Definitions relative to *addition*, *order*, *multiplication* are stated and the relevant theorems proved. *Fractions* are then defined as ordered pairs of natural numbers and the corresponding definitions and theorems given. *Rational numbers* are then defined as classes of fractions and again the corresponding definitions and theorems are given. The rational numbers having been constructed, the natural numbers and fractions have played their part and are no longer required explicitly. *Cuts* are defined as sets of rational numbers, and the former procedure is again followed. Cuts are then called *positive numbers*, and the *negative numbers* and *zero* are now created. The totality of all these are called *real numbers*, and the rational numbers (as previously defined, and as distinct from rational real numbers) are now no longer needed. Dedekind's fundamental theorem is proved. Finally *complex numbers* are defined as ordered pairs of real numbers. After the standard procedure of defining addition, etc., and proving the necessary theorems, the real numbers (as previously defined, the term real being now transferred to certain of the complex numbers) are no longer needed. The canonical representation of complex numbers is established and the foundations of mathematical analysis are complete.

Every student of mathematics is expected to know that the development carried through in this book is possible. But this is, so far as one knows, the only book that gives it completely. It is far better, and just as easy, to follow it right through than to have general vague ideas about it. As the reviewer knows from his own experience, such is the sense of inevitability conveyed by Professor Landau's treatment that if one starts to 'follow' it in lectures one finds it almost impossible to do otherwise than reproduce it word for word. The conclusion is, of course, that the book renders lectures upon the subject superfluous—the author himself believed that a normal student could read it in two days.

W. H. McCrea

BOOKS RECEIVED FOR REVIEW

Inclusion of books in this list does not preclude their being reviewed in later issues

John Baillie, *Natural Science and the Spiritual Life*, Oxford University Press, London, 1951, pp. 43, 5s.

I. M. Bocheński, *Ancient Formal Logic* (Studies in Logic, and the Foundations of Mathematics) North-Holland Publishing Co., Amsterdam, 1951, pp. vi + 122.

Rabbi Ben Zion Bokser, *The Wisdom of the Talmud*, Philosophical Library Inc., New York, 1951, pp. xx + 180, $3.75.

Douglas Bush, *Science and English Poetry*, Oxford University Press, New York (London : Geoffrey Cumberlege), 1950, pp. viii + 166, 16s.

Haskell B. Curry, *Outlines of a Formalist Philosophy of Mathematics* (Studies in Logic and the Foundation of Mathematics), North-Holland Publishing Co., Amsterdam 1951, pp. vii + 75.

H. Dingler, *Grundriss der Methodischen Philosophie*, C. F. Winter'sche Verlaghandlung, Füssen, Germany, 1949, pp. 143, à order. geb. DM. 5.80. kart DM. 4.90.

Karl Dürr, *The Propositional Logic of Boethius* (Studies in Logic and the Foundations of Mathematics), North-Holland Publishing Co., Amsterdam, 1951, pp. x + 79.

E. J. Furlong, *A Study in Memory*, Thomas Nelson & Sons, Ltd., Edinburgh, 1951, pp. vii + 109, 12s. 6d.

C. C. Gillispie, *Genesis and Geology*, Harvard University Press, U.S.A. (London : Geoffrey Cumberlege), 1951, pp. xiii + 315, 30s.

von Ulrich Klug, *Juristische Logik*, Springer-Verlag, Berlin, 1951, pp. viii + 160, DM. 18.60.

Bruno v. Freytag gen. Löringhoff, *Philosophical Problems of Mathematics*, Philosophical Library Inc., New York, 1951, pp. 88, $2.75.

J. Łukasiewicz, *Aristotle's Syllogistic*, Clarendon Press, Oxford, 1951, pp. x + 141, 15s.

Abraham Robinson, *On the Metamathematics of Algebra* (Studies in Logic and the Foundations of Mathematics), North-Holland Publishing Co., Amsterdam, 1951, pp. ix + 195.

E. Schrödinger, *Science and Humanism*, Cambridge University Press, 1951, pp. 68, 8s. 6d.

Manfred Thiel, *Ontologie der Persönlichkeit*, Springer-Verlag, Berlin, 1950, pp. xxiii + 635, DM. 29.70.

N. I. Vavilov, *The Origin, Variation, Immunity and Breeding of Cultivated Plants*. Selected writings translated from the Russian by K. Starr Chester. *Chronica Botanica*, 1951, Vol. 13, No. 1/6, pp. viii + 366 (Chronica Botanica Co., Waltham, Mass., U.S.A. and Wm. Dawson and Sons Ltd., London), $7.50.

John M. Warbeke, *The Power of Art*, Philosophical Library Inc., New York, 1951, pp. ix + 493, $6.00.

P. P. Weiner, *Evolution and the Founders of Pragmatism*, Harvard University Press, U.S.A. (London : Geoffrey Cumberlege), 1949, pp. xiv + 288, 32s. 6d.

Gardner Williams, *Humanistic Ethics*, Philosophical Library Inc., New York, 1951, pp. xii + 223, $3.75.

Henry Horace Williams, *Logic for Living : Lectures of* 1921-22, edited by Jane Ross Hammer, Philosophical Library Inc., New York, 1951, pp. xix + 281, $3.75.

A. Wolf, *A History of Science, Technology and Philosophy*, New Edition prepared by D. McKie, George Allen & Unwin Ltd., London, pp. xxvii + 692, 42s. (First published, 1935 ; 2nd edition, 1950.)

G. H. von Wright, *A Treatise on Induction and Probability*, Routledge & Kegan Paul, Ltd., London, 1951, pp. 310, 30s.

G. H. von Wright, *An Essay in Modal Logic* (Studies in Logic and the Foundations of Mathematics), North-Holland Publishing Co., Amsterdam, 1951, pp. vi + 90.

END OF VOLUME II